Engraved by T. Woolnoth, from an Original drawing by F. Massard.

M. HAÜY.

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THE PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY, AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

THE

PHILOSOPHICAL MAGAZINE.

I. *On the Means of counteracting the Effects produced by the Formation of the Gases found in Coal Mines.*

To Mr. Tilloch.

Sir, I am induced to request you to lay the following pages before the public, from a desire to put a subject of much importance in a train for investigation. Little has hitherto been written on the subject, calculated to do away the frequency of the dreadful accidents to which miners are incident; and what has been written has come from the pens of those who were not every way qualified to do justice to what they have taken in hand.

As the *manner* of working mines varies according to circumstances, many things herein will appear to some as redundant, while to others much will seem to be omitted. But let it be remembered that I lay no claim to general knowledge: what will be advanced is chiefly the result of practical observation in a particular district.

To men of considerable experience in mining, probably, nothing will be found worthy of attention: but if a few individuals should chance to reap any benefit, or should it prove the means of preventing one single accident, my end will, in no small degree, be obtained. With regard to the experimental part, it may be proper to observe, that my situation renders it in many instances impossible to procure apparatus of the best kinds; and hence, if any inaccuracies should chance to be discovered by the scientific reader, it is hoped ample allowances will be made.

In endeavouring to accomplish the important and desirable object before us, we ought to adopt those measures which have a tendency to prevent the accumulation of the noxious

noxious gases; for wherever they are suffered to accumulate, there is continual danger*.

I do not know of any vehicle, neither do I think it possible to devise one, at all comparable to that with which Nature has wisely provided us, namely, atmospheric air.

This offers itself so abundantly, and requires such simple arrangements to convey and apply it to the purpose, that we may be considered as extremely deficient in our duty, if we do not make the best use of it, before we turn our attention to matters of speculation. In fact, no contrivances can supply its place. A salubrious atmosphere is so necessary to the healthy action of the human system, that in mines where a due proportion of its vital principle in a given space is sometimes wanting, it is the cause of numbers lingering out a life of disease†.

Every remedy which tends materially to increase the expense of working a mine, or which throws considerable obstacles in the way of *getting* and *conveying* the minerals out of the mine, cannot on these accounts in the present day be adopted. Of this kind we may reckon the schemes for neutralizing or destroying the gases by chemical agency; such, for instance, as the one of oxy-muriatic acid for destroying *fire-damp*, proposed by Dr. Trotter‡. The expense attending such a process would be enormous, the mode of practice unmanageable, and the remedy, if it could be successfully employed, would be worse than the disease. The quantity of hydrogen gas, or *fire-damp*, evolved during one day in a mine subject to it, is almost incredible to persons unacquainted with the fact; and as we cannot reasonably expect to procure chemical *reagents* at a much cheaper rate than this gas may be obtained artificially, the idea of applying them to this purpose must therefore be entirely abandoned.

There are also other modes which might be adopted, such as procuring *light* accompanied by a heat below the temperature requisite to fire the inflammable gas; or by

* This requires no proof, because accidents can never happen but where there is an accumulation of gas.

The idea of preventing their formation is too ridiculous to deserve the least notice.

† Working in mines, where *fire-damp* is very prevalent, is the occasion of violent headachs, and sometimes of sore eyes. *Choke-damp* is considered as producing asthmas, headachs, &c.

‡ "A Proposal for destroying the Fire- and Choke-damps of Coal Mines, &c., &c., on the Principles of modern Chemistry, by T. Trotter." Newcastle.

forming

forming an insulated atmosphere round the flame of a lamp or a candle; admitting the air of the mine thereto in such proportion, and so diluted with the insulated and incombustible air, as to prevent explosion.

This last plan has often been proposed; and I understand that a gentleman of Sunderland has completed a contrivance of this kind. Although these modes of obtaining the end in view are in a great measure free from the difficulties inseparable from reagents, yet there are others which render them also objectionable.

With regard to the first method, the only one with which I am acquainted is the *steel mill* as it is called, which consists of a number of flints, so placed with respect to a circular steel pulley or wheel, to which a handle is fixed, that, when it is turned, a considerable quantity of light is given out. The manual labour attendant on this plan, together with the expense of renewing the apparatus, (to say nothing of the imperfect light procured,) renders it inapplicable to ordinary purposes. Indeed I believe it is seldom or ever used, except in peculiar situations, when a part of the mine is incapable of ventilation, without great expense or difficulty. Moreover, it cannot be considered as entirely free from danger of exploding the gas; and that this has actually happened, I am well informed.

Reflectors are sometimes used to convey light, in *thrilling* (or thurling) between two *shafts* of no great depth, or for other works of a like nature, where the quantity of *fire-damp* is considerable, and incapable of being carried off with facility.

Concerning lamps constructed so as to burn in an insulated atmosphere, nothing deduced from practice can be said. They must necessarily be expensive, and extremely liable to be broken by pieces of coal, &c., flying from the workmen, or from accidental falls; and should this happen in a part of the mine where the gas was accumulated in a quantity sufficient to render the use of such a lamp necessary, what would be the consequence? Besides, complicated machinery is ill calculated for ignorant and careless miners: an instrument of this nature must needs require nice adjustment and frequent trimming, which last could not be done with perfect safety where any danger was to be apprehended. In short, men who have ever been accustomed to the use of a candle, and a piece of clay as the ready means of fixing it where circumstances require, will not willingly be incumbered with a lamp, even of a common construction. There is another serious objection to

be urged against all the foregoing modes of obtaining light, namely, the pre-supposed want of a sufficient quantity of oxygen in the atmosphere; this, added to the positive bad qualities of the gases present in such situations, would greatly endanger the health and lives of the workmen.

Let us now turn our attention to the more practicable and effectual plan of ventilation; and in pursuing this subject we will commence with the first opening of a colliery.

It should be understood, that hydrogen and carbonic acid gases are often met with in *sinking shafts* of considerable depth, as in many instances such are evolved for a length of time in great quantities from fissures in the strata. If two shafts are sunk near together, *thrilling* occasionally through from one to the other is of great utility; but in some instances this is by no means sufficient to produce the desired effect. Should this be the case, *air-pipes* of large diameter must be extended from the bottom of the sinking shaft, through the last *thril* into the adjoining *shaft*, in the bottom of which, providing it is not deeper than the *thril* last made, (in which case a scaffold will be necessary,) a fire ought to be constantly kept burning: by this means pure air will descend the sinking shaft, carrying with it the extricated gases up the *pipes* into the adjoining *shaft*, whose column of air will be rarefied by the fire. I conceive this method to be superior to that of building a cupola, in which a fire is placed, upon the ascending shaft; because, if the chimney be not somewhat near the size or diameter of the shaft, the current of air will be impeded thereby. Besides, a rarefied column of air in a deep shaft has a greater tendency to produce a quicker current, than the column of air in a chimney, beyond doubt.

When no communication can readily be had with another shaft, it is sometimes customary to partition the sinking shaft with boards down the middle. This, though a simple, is an expensive plan; and if the temperature occasioned by the workmen is the only means of producing a circulation, how must it be carried on, when the men are not at work? I am aware that where there are pumps working, these would, in some measure, remedy this seeming defect, especially by occasionally turning or spilling the water lifted down from the top to the bottom of the shaft, for a short time.

If wooden air-pipes, of about 15 inches diameter, are put down the sinking shaft, and the upper ends of them are fixed to the ash-pit of the engine boiler, (which, for an engine capable of lifting the water of a deep shaft, is of considerable

considerable size,) and the ash-pit is purposely constructed, by being closed in front, and provided with a door, through which the ashes may be removed, all the air required for combustion must necessarily be forced up the pipes out of the shaft, owing to the great rarefaction existing in the ash-pit and engine chimney. The fire bars should in this case be far asunder, and be kept very clean, in order to allow a greater quantity of air to pass through their interstices.

Another plan consists in the use of *blast cylinders*, worked by the engine. Though these are generally constructed so as to force air down the pipes, yet I should recommend the reverse mode, because it will frequently occur that the air will be foul in some parts of the shaft and yet be salubrious at the bottom: besides, the downward blast prevents the conveying feeders or jets of gases from particular fissures directly out of the shaft, by turning them into the air-pipes. Of the methods above detailed, the second, by thrilling, appears to me to have a decided advantage, from its constant and extensive powers of ventilation.

We will now suppose the shaft or shafts to be sunk down, and a direct communication formed in the mine, and shall now touch upon such a course of proceedings as ought to be adopted to ensure the working of the mine with safety.

It is generally customary to begin with *driving* two parallel *headings* or levels, pretty near to each other, intersecting the pillar betwixt them by *thrils*, as often as occasion requires, taking care to make up or closely stop the last but one after them in succession. One of these *levels* is called the *air-way*, or wind-gate; and the other, the waggon-way, or passage through which the produce of the mine is conveyed to the shaft; and these are extended to the boundary fixed upon for the workings.

When other headings are made to communicate with these at right angles, or in any other direction, it is necessary to place a close-shutting door, in order to turn the current of air, out of its course, through one branching heading, and back along another. But there is such a variety in the modes of working mines, each of which is adapted to peculiar circumstances, that it would be tedious to detail them particularly; neither is it absolutely necessary.

As a great deal of expense may be spared and many accidents avoided, in situations where *fire-damp* is prevalent, by

by having these headings properly constructed, it may not be amiss to state generally what should be observed in forming them.

The *first* thing to be attended to is, to have the air- and waggon-ways of a sufficient width and height, so as to admit an abundant quantity of air to carry off the extricated gases; and in proportioning them, it would be well to attend to the extent and number of air-ways, &c., or the area of mine required to be got or worked to a shaft; and likewise to the quantity of gas likely to be evolved. In thin mines of coal, this ought to be an important consideration. No passages or headings, in situations where the workings are likely to be extensive, ought to be less than four feet and a half square. There is a lateral friction occasioned by air passing through apertures of every description, and this is in proportion to the area of their sides, if smooth; and is increased by the roughness and irregularity of the surface passed over, as well as by contractions*. There are other obstructions to a regular circulation, such as the opening of doors, men or horses, &c. &c., passing in a direction contrary to the current of air; and these circumstances render capacious air-ways indispensable.

Secondly. Every part of such air- and waggon-ways should be made as secure as possible; for, when they are left in an unfinished and temporary state, they must needs, ere long, be repaired, at an additional expense, to say nothing of the hindrances and accidents to which such slovenly management is liable.

Thirdly. Thrills should be made nearly as large as the air-ways; a number of contractions of this kind would render a capacious air-way of no service. When they are *made up*, great attention should be paid to their being air-tight, otherwise the air will be lost, and the current thereby considerably diminished.

Fourthly. Doors for the purpose of turning the air should be well made, very tight, and firmly fixed. They are sometimes constructed with springs, &c., so as to shut of themselves; but as the most simple contrivances are found to answer best, these do not generally obtain. There is a method of construction, which appears to me to be capable of answering very well, namely, swing doors, which if properly framed together, and suspended by double centres, will shut of themselves, and open with little trouble;

* Any person may satisfy himself of the truth of this, by noting the regular current of air through the windings of a coal mine, and then causing the air to pass directly between the two shafts.

they

they should be hung at about two-thirds of their length from their bottom end, and counterbalanced so as to act against the current of air. In mines where horses are used, a man will thrust them open without delay, and they will immediately shut after him. Common doors, such as shut one way, are on many accounts the best; but they are apt to be left open in situations where persons are not expressly appointed to attend them, and indeed blown open by the air acting in a contrary direction, owing to the sudden impulses of blasts, or shots fired, &c. &c.

Lastly. Great care ought to be taken to preserve the air-ways, &c. in good repair; and should any part of them give way, no time must be lost in securing them, and removing the rubbish, that there be no obstruction to free ventilation *.

When all the headings are completed, or as many of them as are deemed necessary, it is customary to proceed in working the mine; but, for reasons already stated, it would be departing from my original intention to say anything upon this head.

Indeed, to an intelligent miner, who has an abundant supply of air at his back, little difficulty will arise in turning it effectually to his purpose.

Ventilating the *waste* or old *workings*, so as to prevent an accumulation of damp, is a matter of importance. In thin mines it is not necessary to do it for any length of time, as the *roof* and *floor* soon gradually close together: in others, where the *roof* is rock, and of such thickness as to stand under any circumstances, it may be practised with ease: it is in mines of four feet thick and upwards, whose superior strata are not capable of supporting themselves for a considerable width, that the greatest difficulties arise. To leave sufficient pillars of coal would frequently be to bury the profits of a colliery, and at the same time destroy an amazing quantity of valuable fuel.

Thin pillars are in general required to ventilate the workings, and these are for the most part left: where they happen to stand until a *general weight* or settlement of the

* I have known instances, where the air-ways have been so much neglected in this particular, as to put a complete stop to the circulation of air; and, as might very naturally be expected, several misfortunes happened in consequence. The ill effects attending such unpardonable inattention are almost incalculable:—loss of men's lives, loss of time, expense incurred in repairs rendered more difficult to accomplish from damages sustained by explosions; the invariable advance in men's wages, as some compensation for the great dangers to which they are exposed, may all be placed to this account.

superior

superior strata takes place, and the *waste fills up*, it is well, as then no damp will accumulate, and thereby endanger the lives of the neighbouring workmen.

Too much caution cannot be observed, by men who are working near such *old hollows*, never to introduce their candles into any cavities which may happen to be penetrated, but immediately extinguish them and retire, until proper means can be adopted to ensure safety. What accidents have occurred for want of this simple precaution *!

It may be, and in fact is, at times necessary, owing to increase of temperature or levity in the atmosphere, to obtain a greater supply of air than is wanted in the ordinary course of things, or, more properly speaking, to preserve an uniform circulation.

Here we have an admirable contrivance, whose powers are unlimited; and this consists in rarefying the column of air in the ascending shaft by means of a fire. There appears to be a difference of opinion as to the best mode of accomplishing this. Some build a chimney or cupola upon or near to the shaft, making the top thereof so close that the air must pass up the chimney, at the bottom of which a fire is placed: others hang, by means of a chain, &c., a circular grate filled with burning fuel, down the shaft; and some fix a grate in the bottom of the shaft, in a heading, driven a few yards for the purpose; while others cut a groove in the shaft side, which, when built up in front, forms a chimney.

If we are to judge of the comparative merit of the foregoing plans from their respective powers of rarefaction, we must give to the last method but one a decided preference. By it we are enabled to produce the greatest difference in weight between the two columns of air, on which ventilation solely depends; and if the grate be placed a tolerable height, and opposite to the current of air passing into the shaft, we shall obtain the quickest draught for the fire. The chimney not only greatly increases the effects of the fire, but is a sort of guard to the ropes, which might be damaged by coming too near the naked fire.

I shall conclude this imperfect sketch, with offering a few general remarks, which, to those who put them in practice, will not be found useless.

In all cases where danger from fire-damp is to be apprehended, the miners should never work naked. It is not

* This is a very common trick of miners, owing to an idle curiosity; and but too many have paid dearly for their temerity and want of consideration.

so

so much the *depth* of the *burn* as the *extent* of it, which proves fatal: a covering of thin flannel would be found almost invariably to confine the injury to the face and extremities. Some miners cannot afford to do this, and others will not, considering clothing as an incumbrance; but where men's lives and the interest of the proprietors are so much at stake, it ought to become a matter of compulsion.

Should it so happen, that through some unforeseen accident an explosion takes place, the persons in immediate danger ought to throw themselves down, covering themselves as well as the spur of the occasion will permit, and then with all speed make their nearest way to that part of the mine from whence the air proceeds, taking care to creep as low as possible: by this means, suffocation from the *azote*, which remains after combustion, will be avoided. Should any one be so unfortunate as to fall thus for want of respirable air, or by the choke-damp, every exertion ought to be made to cause the air to circulate through the place, so as to enable the sufferer to be brought out; and when this is effected, resuscitating means should be immediately employed. Instances often occur, of men being stifled by the azotic gas, who were little or none the worse from the effects of the explosion.

In every instance, where a miner penetrates any new or neglected cavity whatever, the precautions before mentioned ought to be strictly observed, whether the mine be much subject to *fire-damp* or not.

The practice of laying scaffolds over or in a shaft, in order to do some necessary repairs, without being cautious to ventilate the underside, should be carefully avoided; terrible explosions have been known to follow this negligent practice.

These and other similar remarks, which will occur to every one conversant with mining business, if properly modified, would form a code of rules, with which every miner should be acquainted.

December 19, 1809.

*** We shall at all times be glad to receive papers of the above description from practical men, on the subject of their pursuits, and shall think little of our trouble in rounding a period, or correcting grammatical inaccuracies, for such as may chance to need this assistance, owing to the devotion of their time to other important objects.——EDITOR.

II. *On*

II. *On Expectorated Matter. By* George Pearson, *M.D., F.R.S. Abridged from the Philosophical Transactions for* 1809.

"The attention of physiologists has been very much withdrawn, for the last half century, from the consideration of the different states of the circulating and secreted fluids, in consequence of the opinion that the nervous and fibrous or muscular systems can afford satisfactory interpretations of the phænomena of living beings; and on account of the disgust produced by the visionary properties and groundless hypotheses, originating in the humoural doctrines of Galen. But late experiments have manifested, that various things taken into the stomach can be made at pleasure to produce considerable effects, by impregnating sensibly the blood and urine, as well as the milk, sweat, and perhaps saliva. Further, the fine experiments of Professor Colman have shown, that the contagious glanders may be excited in the ass by the transfusion of the blood of a glandered horse, and the matter from the nose of the glandered ass can produce this disease in the horse or the ass*. Hence I apprehend it is reasonable to expect, that the further investigation of the properties of the animal fluids will afford gratifying instruction to the researcher in natural science, and important practical information to the physician.

"On the present occasion, I desire the honour of communicating the knowledge I may have acquired, by investigating the properties of expectorated matter secreted by the bronchial membrane. The appearances of this substance serve to regulate the judgement of the physician concerning several diseases of the lungs; but especially of that of pulmonary tubercles, which yearly destroys 120,000 to 140,000 subjects of the United Kingdom. It is fit that I remark, that I do not notice in this paper the ingenious experiments of several learned chemists, because by so doing I should be led into a detail of too great extent for my design.

"The numerous varieties of expectorated matter, according to my observation, may be arranged and characterized under the following seven heads:

"I. The jelly-like semi-transparent kind of a bluish hue, excreted in the healthy state.

* Mr. Colman alleges, that there is not a sufficient quantity of blood, in a single glandered ass, to excite the glanders by the transfusion of blood into the horse.

"II. The

" II. The thin mucilage-like transparent matter, so copiously expectorated in bronchial catarrhs.

" III. The thick opaque straw-coloured, or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections; especially in that of tubercles.

" IV. Puriform matter secreted without any division of continuity, or breach of surface of the bronchial membrane, very commonly occurring in pulmonary consumptions.

" V. The matter which consists of opaque viscid masses, together with transparent fluid; or the second sort above stated, with nodules of the third or fourth kind.

" VI. Pus from the vomicæ of tubercles.

" VII. Pus from vomicæ by simple inflammation of the lungs, and without tubercles.

" Other kinds of matter are occasionally coughed up, such as calculi—masses of self-coagulated lymph—serous fluid—blood itself—and perhaps the vascular substance of the lungs; but I do not write on these matters, because they either do not belong to any particular recognized disease; or they are rare occurrences in some well known disease, and are too obvious to require description.

" § I. *Sensible or obvious Properties.*

" 1. *The jelly-like matter*, as already said, is excreted in the best health, as well as sometimes in disease. It is mostly coughed, or hawked up, in a morning soon after a night's repose, during which it seems to accumulate. A few masses, or nodules, then appear of the consistence of jelly, and of the size of a pea to a hazle nut. It is also at any time liable to be excreted, in consequence of various extraneous matters irritating the fauces, to the amount of a few nodules. It is of a grayish colour or inclining to blue, with black specks; and it is rarely whitish in nodules. The consistence is that of jelly, but of much greater tenacity. It has a barely perceivable taste of common salt, or muriate of soda. It commonly floats on water, but by agitation to disengage air bubbles, it sinks. It has no smell. To the naked eye, or assisted by a single magnifier, this matter seldom appears uniform, but consists of a mixture of opaque and transparent masses of irregular figures. With the compound microscope, spherical particles were perceived, though few in number, when duly diluted. The presence of an alkali I could in no instance perceive, by means of the usual tests, namely, turmeric paper,

paper, litmus paper slightly reddened by vinegar, and cloth stained with violet juice; nor was an acid denoted by means of litmus paper, except when I had reason to believe it was derived from various acid substances taken with the food, or drink, adhering to the inside of the mouth and fauces.

"2. *The mucilage-like expectorated matter*, according to my observation, occurs much less frequently than the other sorts. It appears suddenly in great abundance in certain bronchial catarrhs. I have seen it to the amount of two or three pints in twenty-four hours. It is also secreted, but less copiously, in paroxysms of spasmodic asthma, and of the hooping-cough; and but rarely in pneumonic or pleuritic inflammations, and in some chronical organic diseases of the heart and lungs.

"This matter is a transparent uniform fluid of the consistence of white of egg; or of a mucilage compounded of about one part of arabic gum, and four or five parts of water. It is colourless—has a fleshy smell—has a brackish taste. After standing eight or ten hours, a deposit takes place of fibrous, leaf-like, or curdy masses, some of which are seen suspended in the clear fluid. In some cases nodules of opaque thick ropy matter, at certain times, accompany this mucilage-like matter. Under the simple magnifier I perceived irregular figured masses partly in motion and partly suspended. With the microscope, globules were seen; but larger considerably than those of the blood, and much less numerous. With the usual tests there were no indications of alkali nor of acid, provided the matter was unmixed with other things. It usually floated, or was suspended in water, when first expectorated; but on standing in the water it fell to the bottom, evidently owing to the disengagement of air-bubbles.

"By standing exposed to the air in warm weather, it sooner grew fœtid than pus of abscesses; without becoming opaque. Neither could I render it opaque or thicker, by exposure to a stream of oxygen gas for an hour; or by exposure of it in a jar of this gas for a month.

"3. *The opaque ropy matter* above mentioned.

"1st. It is secreted most copiously in that very common, and extensively epidemial disease of our climate, the *winter-cough*, occasioned by tubercles, to the amount of half a pint to a pint in twenty-four hours; especially during the winter season for several successive years, and sometimes during the whole of a long life, after the age of forty or fifty years. 2dly. It is often the expectorated matter of the

the pulmonary consumption of young persons, also occasioned by tubercles, but frequently mistaken for the pus of abscesses or vomicæ.. 3dly. It appears, oftentimes, in pneumonic or bronchial inflammation with fever, seemingly being a beneficial discharge; as well as in some instances at the close of a fever without concomitant inflammation of the lungs. 4thly. A severe paroxysm of spasmodic asthma is often terminated in the excretion of this kind of matter. 5thly. A secreted substance of this sort is sometimes expectorated in various chronical organic diseases of the lungs, the heart, aorta, and parts contiguous to the lungs, which occasion difficult transmission of blood through them.

"In all these instances the matter by expectoration is of the consistence of thick cream, or of thin toasted cheese; so tough as to hang in the form of a rope, four or five inches in length, on pouring it from one vessel into another. Its aggregation is such that it is readily detached in large masses from the vitreous surface of vessels. It is not unusual for small black, or reddish spots, and streaks, to appear on the surface of this sort of expectorated substance. A pretty large bulk of it is seldom throughout uniform; but it is frothy, and exhibits opaque masses of various hues with transparent matter interposed. The colour is yellowish, straw-coloured, and white, or gray: it also, though seldom, is greenish and blueish. The taste asserted by patients, is, in their own terms, various, namely, saltish, nasty, faintish, sweetish, luscious, or like that of a sweet oyster,—a sharp or sour taste is the most rare. The only smell which I have perceived is that of flesh, but very frequently there is none. When any offensive or pungent smell was perceived, immediately after expectoration, I have always found that it was owing either to the foulness of the vessel in which it was received; or it was from extraneous matters in the mouth, and from decayed teeth.

"This opaque viscid substance, being duly diluted with distilled water, was examined with microscopes of common as well as of very great powers: by means of any of them crowds of spherical particles were seen passing to and fro, in currents, not unlike those of the blood; except that they were larger. These globules I could not destroy, nor alter in form, by trituration; nor by long boiling in water; nor by exsiccation, and again dissolving in water; nor even by coagulation with mineral and vegetable acids, with alcohol, with sulphuric ether, or with tannin, and alum; nor by mixture with caustic alkalies in a proportion which leaves the

liquor turbid; nor for some time after the putrefactive process had appeared. But these globules disappear with such a proportion of sulphuric acid as detaches charcoal; or of nitric acid, and of liquid potash, as produce a clear solution: also by charring by fire. It is perhaps superfluous to remark, that these atomic globules are quite different from the air bubbles usually entangled in this kind of matter, as perceived by the microscope; the latter differ much from the former, in being of far greater magnitude—in being less numerous—in being transparent, and disappearing on agitation, or heating the matter, or even by mere standing.

"For the most part this expectorated substance swims on water; but by agitation or stirring to disengage air bubbles, or by merely standing, it sinks. Some of the lumps suddenly hawked up, immediately fall to the bottom of a vessel of water. No signs of either acid, or alkali, appeared on the trials of this matter with well known reagents, provided it was free from extraneous matter; but it was apt to betray acidity from things taken with the food or drink.

"4. *Puriform matter.* I have seen this matter expectorated in several diseases in the quantity of two or three ounces to half a pint in twenty-four hours, on some rare occasions, without any breach of surface. I believe it would be considered by every one to be *pus*, having the properties commonly admitted to be those of this substance. It will, however, perhaps, only be just to call it *puriform*, for the present, as it appears to me probable, that I shall hereafter be able to show that it possesses properties not belonging to pus of abscesses, although in the obvious, or sensible properties, it is similar to such pus. Accordingly this expectorated matter is not only opaque, white, or yellowish, and thick as the richest cream, but it also has not more tenacity than cream. It is not apt to entangle air, and therefore it immediately mingles with water, rendering it milky; and presently subsides to the bottom, leaving the water clear, or at least whey-coloured. It appears to the naked eye uniform in its texture; and nearly so under the simple lens: but under the microscope thousands of globules similar to those of the blood are seen, which are indestructible as those above related belonging to another kind of expectorated matter.

"The substance, of which I am now speaking, is most frequently excreted in the latter stages of pulmonary phthisis, for many weeks successively. It is taken for granted that this matter is from a breach of surface or ulceration; but

but on examination after death, such a state was not found, in many instances, under my observation, although the lungs were as usual full of tubercles and vomicæ. This puriform matter is occasionally expectorated in certain other diseases. The last summer my colleague, Dr. Nevinson, furnished me with several ounces of this sort of substance, but of a greenish hue, and of the consistence of thin cream; which was expectorated by a woman in the third week from the attack of the measles. In a few days she died. On examination of the lungs very carefully, by the excellent house surgeon of St. George's hospital, Mr. Dawes, no ulceration could be discovered in the trachea or in the bronchial tubes; nor were any tubercles or abscesses found in the lungs. The patient, according to my information, had expectorated more than a pint of this fluid every twenty-four hours for a week before death. In another hospital case, a man laboured under a cough with spitting of matter, which all who saw it called pus, and as usual it was considered to arise from an ulceration, or suppurated tubercles; but, on examination after death, the disease was ascertained to be condensation of the lungs, to the consistence of liver, with water in the cavities of the chest, and nothing more.

"5. *Opaque viscid matter of a third, and perhaps fourth sort*, above distinguished, appearing in nodules, and irregular-figured masses, *mixed with transparent slimy matter of the second sort.*

"It is not unusual to see the mixture of these two different kinds, from severe fits of coughing in that constant epidemy of the British islands, the winter chronical pneumonia.

"Different parts of the bronchial membrane being in different states, may account for the secretion of the two different matters. This seems more probable than that these different matters should be secreted from the same part; although it is true that the same part does secrete at one period transparent thin slime, and at another an opaque thick matter. The former is occasioned by great irritation of the membrane, and the latter is the effect of a more gradual secretion with much less irritation.

"For the sake of brevity, I avoid a further description. The practical application of these observations, however important, would not be suitable in this place.

"The sixth and seventh kinds of expectorated substances being secreted after a quite different manner, and being very different in their nature from the preceding five kinds, I shall not give an account of them in this paper."

[The author then describes at large, and with much precision, the effects produced on expectorated matter by the agency of caloric; of alcohol of wine, of water; and of acetous acid—also some experiments with different objects from any of these: but we confine the remaining part of the present extract to his *Conclusions*, as containing that kind of information which will be most acceptable to the generality of our readers.]

" *Conclusions.*

" 1. From the preceding experiments and observations, and from others which I might have related, it does not appear that the various kinds of expectorated matter, page 12, differ in the ingredients of their composition, but merely in the proportion of them to one another.

" 2. It has been shown that expectorated matter consists of coagulable, or, as it is also now frequently termed, *albuminous* animal substance, and of water impregnated with several saline and earthy bodies;—that the largest proportion of the animal substance, which may justly be called an oxide, amounts to one-twelfth, and in some very rare cases to one-tenth of the expectorated matter, reduced to a brittle state by evaporation; and that the smallest proportion of this oxide, in rare instances, amounts to one forty-fifth of the expectorated matter; but that the usual proportions of it vary between one-twentieth and one-sixteenth of this coagulable oxide to the evaporable water, that is, between five and six per cent. of the expectorated matter.

" 3. The impregnating substances have been shown to be muriate of soda, varying commonly between one and a half to two and a half per 1000 of the expectorated matter—Potash varying between one-half and three-fourths of a part per 1000—Phosphate of lime about half a part of 1000—Ammonia, united probably to the phosphoric acid; phosphate, perhaps of magnesia; carbonate of lime; a sulphate; vitrifiable matter, or perhaps silica; and oxide of iron. But the whole of these last six substances scarcely amounting to one part in 1000 of the expectorated matter, it would be useless to estimate the proportion of each of them. It is very probable that the proportions and quantities of these ingredients vary much more than now represented in different states of disease and health *. It is very probable also, that some of the ingredients may occasionally be absent,

* In one case, the opaque expectorated matter in a pulmonary consumption having been exsiccated to brittleness, became almost liquid after a night's exposure to the air.

sent, and others of a different kind be present, agreeably to the different states, on different occasions of the other secretions.

"4. It is manifest that the different states of consistence of expectorated matter are owing to the proportion of albuminous or coagulable oxide; but I purposely avoid giving an account of the different conditions of health, on which the differences of consistence depend.

"5. The thicker the matter, the smaller I commonly found the quantity of saline impregnation. Hence, in sudden and copious secretions of the bronchial membrane, the matter is asserted to be salt, and to feel hot. In such instances, the proportion of coagulable matter was small, but that of the saline impregnations, particularly of the muriate of soda, and neutralized potash, so great, that the exsiccated expectorated substance tasted very salt, and presently grew moist, or even partially deliquesced; but the opaque ropy or puriform matter afforded a much larger proportion of exsiccated residue, which was but slightly salt, and generally only became soft on exposure to the air. This property of growing moist depends upon the potash.

"6. Each of the human fluids, according to my experiments, contains neutralized potash; at least, this is the fact of the blood, dropsy fluid, pus of abscesses, and pus secreted without breach of surface; the fluid effused by vesicating with cantharides; the urine; and in course in the very abundant secretion from the nose by a catarrh. The alkali being united to oxide of animal matter in these fluids, it is easily demonstrable.

"7. Although I think I have discovered many properties by which expectorated secretion may be distinguished from expectorated pus, I shall not speak of them, on this occasion, further than just to observe that the saline impregnation of pus, particularly that of potash, and muriate of soda, is in very much less proportion than in expectorated secretion; and hence it does not become moist after exsiccation, on exposure to the air.

"8. It has been, I believe, uniformly asserted, that the circulating and secreted fluids are impregnated with soda; that it is especially in the matter secreted by the bronchial membrane. The experiments of others must confirm or disprove mine. It seems, however, much more reasonable, that the human fluids should be found to contain potash than soda, united to some oxide or destructible acid; because the former alkali is daily introduced with the vege-

table food, and with the drink of fermented liquor; and it is as little likely to be destroyed, as the muriate of soda also induced in the very same way. But our food and drink do not, commonly at least, contain the soda united to a destructible acid, or an oxide.

"9. It is plain, from the preceding experiments, that expectorated matter belongs to the class of coagulable fluids, and not of gelatinizable, or, as commonly asserted, mucous fluids. It differs from the coagulable fluid, serum of blood, in forming a much thicker fluid with a much larger proportion of water: for serum, and also the water of blisters, is quite liquid, although they afford, on exsiccation, one-twelfth to one-eleventh of their weight of brittle residue, while some kinds of expectorated matter, of the consistence of mucilage, afford only one-fortieth of dry residue, and others of the consistence of thin paste afford only one-fourteenth of residue.

"10. But for the unavoidable extent of this paper, I should trouble the learned Society with various other conclusions and remarks, especially concerning the *globularity* of expectorated matter, which seems to indicate organization. Although Antonius Van Lewenhoeck, above a century ago, discovered the globularity of the blood, and even noticed it in other animal fluids, neither he, nor any other person, as far as I know, investigated the subject in any fluid but the blood, till by Mr. Home's acuteness and industry, at a very early period of life, it was observed in pus. I have in this paper related, that expectorated matter, especially the opaque ropy kind, as well as the puriform, is full of globules, and that, except by such agents as destroy charcoal, they are scarcely destructible. Do these spherical particles consist chiefly of organized carbonaceous matter?"

III. *Memoir on the best Method of decomposing the Chromate of Iron, obtaining Oxide of Chrome, preparing Chromic Acid, and on some Combinations of the latter. By M.* VAUQUELIN *.

WHEN I made my first experiments on chrome, I had such a small quantity at my command that it was impossible to vary them so as to bring all its properties before my view.

* From *Annales de Chimie*, tome lxx. p. 70.

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The importance, however, of the oxide of chrome, on account of its beauty and solidity as a green pigment for earthenware, and in forming imitations of emerald, added to the discovery of large quantities of chromate in the department of the Var, determined me to resume the subject, and to study the properties of chrome at greater length. In the experiments about to be detailed I was assisted by M. Robiquet, an eminent chemist of Paris.

Process for decomposing the Chromate of Iron.

Chromate of iron is generally employed in order to procure a large quantity of the oxide of chrome: this ore has for its matrix a kind of steatite, which, from its colour and some other physical properties, might be confounded to a certain extent with chromate itself, the more easily because these two substances at first sight seem to form one and the same mass: after a little attention, however, we find that the matrix is composed of long and pearl-like laminæ, whereas chromate of iron is very fine grained, shining, and denser than the matrix.

I had formerly employed, in preparing the oxide of chrome, three parts of nitre to one of the ore reduced to fine powder; but I have since found this proportion was by far too large: indeed, as we can isolate the matrix but very imperfectly, it happens that the nitre, by means of the alkali which it sets free, attacks not only the chromate of iron, but also the alumine and silex, which are there in large proportions; and thus the chromate of potash is mixed with an alkaline solution of all these earths, from which two inconveniences result: in the first place, we are under the necessity of employing, in the separation of these earths and the saturation of the excess of alkali, a great quantity of nitric acid, and if it happens that we go beyond the quantity of acid necessary to the exact saturation of the potash, we redissolve a portion of silex, but principally of alumine: in the second place, these earths, on being precipitated, carry down with them chromate of potash, which cannot be freed from them by washing: a third inconvenience occurs, particularly when we operate on a large scale, and when the heat is necessarily long continued, —this excess of alkali attacks the crucible and melts it. Thus the œconomy and success of the operation require that we should employ only one half part of nitre to one of chromate: by these means the mass does not enter into fusion, and the chromate is well attacked. It has often happened

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that the potash has been entirely saturated with chromic acid.

This decomposition being effected, the mass is to be well lixiviated, the residue is then to be treated, when hot, with muriatic acid diluted in water, which takes up the iron, magnesia, alumine, and silex, divided by the action of the potash and the subtraction of the chromic acid.

The solution being terminated, the acid liquor is speedily decanted, otherwise it would go into a jelly, and it would then be very difficult to separate the undecomposed chromate: the latter is to be once more treated as at first; but instead of employing the same quantity of nitre, a fourth part will be sufficient. When the chromate of iron is entirely decomposed, we mix the solutions of alkaline chromate, in order to saturate them by the nitric acid, after which it is proper to crystallize this chromate, as well in order to separate some portions of earth which would have been dissolved by the excess of acid, as to take up a little chromate of iron, which is separated in brown dust by the progress of evaporation. We redissolve in water, filter, and precipitate the liquor by a solution of nitrate of mercury at the *minimum*, containing the least possible quantity of acid in excess.

Even supposing that the chromate of potash has been purified as we have indicated, *i. e.* that it contains neither earthy substance nor muriate, the chromate of mercury is precipitated in a more or less intense colour, according to the state of concentration of the solutions, their temperature, and excess of acid. In some circumstances the molecules of this salt, by approaching each other more slowly, assume more aggregation, even crystallize, and thereby acquire a deeper red colour. We may also remark that the first portions precipitated are the palest, because, in proportion as we subtract chromic acid, it has the same effect on chromate of potash as if we diluted the liquor. To conclude, the colour has no influence on the quality of the chromate of mercury.

When the mercurial solution is employed at the *minimum* of oxidation, and as neutral as possible, there remains nothing in the mother water except nitrate of potash and nitrate of mercury when a superabundance of these has been introduced: but in general these mother waters retain an amethyst colour, and yield with the alkalis a pale green precipitate, which, when heated, leaves oxide of chrome.

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I have attentively examined this precipitate, in order to become better acquainted with its nature and properties.

When treated cold with caustic alkali it dissolves, and communicates a fine green colour to the liquor: a red powder remained which presented all the characters of oxide of mercury at the *maximum*. The alkaline solution, being filtered and subjected to ebullition, deposited a great quantity of green flakes of oxide of chrome, and preserved a fine golden yellow colour: this was chromate of potash.

Having diluted in water a portion of the precipitate obtained by the alkali of the above mother waters, I remarked that it was composed of two different substances; the first was flaky and light; the second, which always occupied the bottom of the mixture, was formed of small crystals of a violet brown. These last presented the following properties: 1st, When thrown on burning charcoal, they are entirely volatilized and condensed in small purple needles on such cold bodies as are exposed to their fumes. 2d, When heated more slowly in a retort, they furnish mercury, and leave green oxide of chrome as a residue: they are dissolved in weak nitric acid, communicating to it a fine yellow colour: if we pour into this solution nitrate of mercury at the *minimum*, common chromate of mercury is precipitated. 3d, When we treat this substance by a caustic alkali, the latter acquires a yellow colour, and a red powder remains which is oxide of mercury at the *maximum*, while the common chromate of mercury gives black oxide by the same process.

The greenish precipitate obtained by the saturation of the mother waters, by means of an alkali, contains therefore green oxide and chromate of mercury at the *maximum*.

From what is above stated, we may easily explain what takes place when the mother waters, although containing mercury in excess, nevertheless give a precipitate of chromate of mercury by the addition of fresh nitrate. The reason of this is, that although the nitrate of mercury at the *maximum* precipitates the chromate of potash, it requires but a very small quantity of acid in order to be dissolved: this inclined me to think formerly that this precipitation had not taken place, because the mercurial solution, containing always an excess of acid, and being rarely at the perfect *minimum*, the chromate at the *maximum*, on account of an excess of acid, is kept in solution; but when we add a fresh quantity of nitrate of mercury, the portion at the *minimum* takes off the chromic acid from the red oxide which is deposited: if

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on the contrary we add alkali, we precipitate this chromate at the *maximum*.

It now remains to explain how oxide of chrome is found in this precipitate:—the following experiment seems adapted to give a demonstration of it. When we treat chromate of mercury at the *minimum*, by the nitric acid, the solution is effected without the extrication of nitrous gas; but if we reduce the quantity of alkali necessary for the saturation of the acid, we obtain in the first place a brownish-red sediment formed of chromate of mercury at the *maximum*: the solution by this subtraction becomes green, and precipitates, on the addition of a fresh quantity of alkali, green oxide of chrome, which is easily redissolved in an excess of caustic alkali. In this case, to all appearance, a portion of the chromic acid is de-oxygenated in order to hyper-oxidate the mercury, from which result chromate at the *maximum* and oxide of chrome.

It is therefore certain that the chromate of mercury at the *maximum*, found in the mother water, may arise from two causes: either it results from the mercurial solution, if it contains oxide at the *maximum*, or it proceeds from the solution of the nitrate at the *minimum*, on account of the excess of acid, and then it is found mixed with oxide of chrome.

The solubility of the oxide of chrome in the alkali furnishes the explanation of what passes, when on lixiviating cold, the product of the decomposition of the chromate of iron by nitre, we obtain a green liquor which becomes yellow on ebullition; this is because the green oxide is deposited, which lays us under the necessity of filtrating these lixiviums before heating them, in order to separate this oxide in the state of purity. This last phænomenon inclines us to think, with M. Godon de St. Memin, that the chromate of iron, so called in commerce, contains chrome in the state of oxide; for it is improbable that the chromic acid is reduced at the same time with the nitre. What still confirms the opinion of M. Godon is, that the acids extract a green oxide only from the chromate of iron.

To return to the subject. We shall observe that it is essential to wash the chromate of mercury with a good deal of water, in order to free it completely from the nitrate of potash, which, by the calcination of mercurial chromate, would again form chromate of potash, which produces a commencement of fusion in the oxide of chrome, consequently gives it a deeper shade, and renders it heavier, which

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we may avoid when the chromate of mercury is prepared with proper care

It is sufficient, in order to obtain the oxide of chrome very pure and of a very fine colour, to heat strongly in a well luted earthen retort the pure chromate of mercury, until no more oxygen is extricated, and to keep up the fire so much the longer in proportion to the depth of the shade we wish to obtain: it seems that there really exist two kinds of oxide of chrome, for by heating it a very long time the green is so weakened, that it passes to a dead-leaf yellow.

Combination of the Chromic Acid with Barytes.

In order to prepare the chromate of barytes, we employ with success chromate of potash well purified and very neutral; we mix with it nitrate of barytes until no more precipitate is produced: we must collect the latter, decant the liquor, and wash several times, until it is entirely freed from all extraneous saline particles.

No harm is done by employing great quantities of water, even warm, in order to wash the salt, for it is not very soluble.

Analysis of the Chromate of Barytes.

Five grammes of this salt, dissolved in the nitric acid and precipitated by sulphuric acid, gave four grammes and four tenths of sulphate of barytes. Admitting with M. Klaproth 68 parts of barytes in 100 parts of sulphate, we shall have, in 100 parts of chromate of barytes, 62·2 of base, and 37·4 of acid.

According to this account, the five grammes of chromate of barytes contain one gramme 87 centiemes of concrete acid, and the latter converted to the state of green oxide by a strong calcination, is reduced to one gramme 56 centiemes, which gives a difference between the quantity of oxygen contained in 100 parts of oxide and 100 parts of acid of 16·6, *i. e.* in order to convert 100 parts of oxide of chrome into acid, we must combine with it 16·6 of oxygen.

In order to effect the analysis of the chromate of barytes, we must dissolve it in weak nitric acid, aided by a little heat, and pour into the solution sulphuric acid in excess; we then wash the sulphate of barytes, and weigh it after having dried and calcined it.

With the same view, we may gently evaporate the liquor to dryness, in order to drive off the nitric acid; redissolve the residue in water, saturate it with ammonia,

and calcine strongly in order to evaporate the sulphate of ammonia and completely decompose the chromate of ammonia: lastly, weigh the oxide of chrome. The latter, when boiled with nitric acid, ought to give no sign of sulphuric acid with the nitrate of barytes.

If the proportions of the chromate of barytes which we have obtained are very exact, as there is every reason to believe they are, the same analysis may serve for those of other chromates which are soluble in water.

Process for obtaining the Chromic Acid pure.

Among the various methods which may be employed for preparing this acid, the most preferable, in our opinion, consists in decomposing the chromate of barytes by the sulphuric acid: all other processes were attended with more or less difficulties.

We must therefore dissolve the chromate of barytes in weak nitrous acid; afterwards carefully precipitate it by means of sulphuric acid, so that all the salt may be decomposed without the sulphuric acid being in excess. If by chance we exceed this point, we must separate the superabundance of the acid by barytes water. We shall then find whether we have seized the point at which the precipitate formed by chromic acid in barytes water is redissolved entirely in nitric acid, and at which the sulphuric acid does not disturb this chromic acid.

We then filter the liquor and carefully evaporate it, particularly towards the latter part of the operation, that we may not decompose the chromic acid: this evaporation must be repeated several times to dryness, in order to expel all the nitric acid.

When the chromic acid is highly concentrated, irregular (*mammelonnées*) masses are formed, in which we see small red crystals grouped together, but they are not permanent in the air, as they attract humidity from it.

The chromic acid thus purified is of a deep red colour, has a very acid, but austere and metallic taste; it is soluble in alcohol, which speedily decomposes it, for the solution becomes green.

Easy Method of determining the Quantity of Chromic Acid combined or mixed with several Saline Substances.

We must put into the solution containing chromic acid a slight excess of nitric acid, if the salt be not of itself soluble nor acid: we must then pour in a little hydro-sulphuret of ammonia, and macerate for some time in a close flask, after

after which we must boil in order to drive off the superabundant sulphuretted hydrogen, filter in order to separate the sulphur, pour into the filtered liquor some drops of caustic potash, and thus obtain the green oxide of chrome; filter again, wash carefully, dry and calcine carefully. On adding to the quantity of oxide obtained the 16·6 centiemes of its weight, we have the quantity of chromic acid which any given salt contained. We may easily see that in this operation the sulphuretted hydrogen makes the chromic acid pass to the state of green oxide, which the excess of acid takes up again in proportion as it is precipitated: if some portions of it remain, we succeed in separating it from the sulphur by ebullition, which serves at the same time to drive off the sulphuretted hydrogen. The potash afterwards added only serves to decompose the salt of chrome which is formed.

Action of Sulphurous Acid on Chromic Acid.

If into chromic acid we pour a great quantity of sulphurous acid, its yellowish-red colour passes to a dirty brown; and if at this period we pour caustic alkali into the liquor, we obtain a reddish-brown precipitate, which is dissolved in the acids.

A greater quantity of sulphurous acid immediately changes the red colour of the chromic acid to a pale green. Thus it should seem that two kinds of oxide of chrome exist, but which scarcely differ in the quantity of oxygen*.

Action of the Acids on Oxide of Chrome.

In general the oxide of chrome obtained by the calcination of the chromate of mercury is attacked by the acids with very great difficulty: we at length succeed, however, in dissolving it; but in order to form combinations we made use of the oxide obtained from the decomposition of the chromate of potash by means of the hydro-sulphuret, according to the process above described. We have seen that this oxide, when recently precipitated, is dissolved with the greatest facility even in the weakest acids.

The sulphate of chrome presented nothing remarkable; we merely observed that it is easily decomposed by heat; for, when slightly calcined, it is no longer redissolved in water.

* We obtain an oxide similar to that which is produced by sulphurous acid in the former case, by passing oxygenized muriatic acid into an acid solution of green oxide of chrome, and into which we gradually pour a little potash.

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The muriate is so far remarkable, that when evaporated to dryness, it gives a red powder which attracts humidity from the air; its solution is of a fine green colour. If we calcine it rather strongly, it gives out a smell of oxygenized muriatic acid, acquires a great bulk, and is transferred into small micaceous brilliant yellow fibres: finally, if we heat it still more, it is totally converted into green oxide.

We boiled several times, and in large quantities, nitric acid over oxide of chrome recently precipitated, and it was completely dissolved; but when we separated the oxide by means of caustic potash, the supernatant liquor was colourless: the contrary is the case when we evaporate to dryness and slightly calcine: by redissolving in water, the latter assumes a reddish colour; and after the separation of the oxide, the liquor remains of a fine golden yellow.

We also dissolved oxide of chrome in phosphoric and in oxalic acids: the first combination was of a splendid emerald green colour, and the other, when viewed in the mass, presented an amethyst hue. Sulphurous acid also dissolves oxide of chrome with great facility.

Action of the Caustic Alkalis on Oxide of Chrome.

If into a solution of chrome a little diluted we pour caustic potash in a quantity above what is necessary for the saturation of the acid, the oxide is redissolved in this alkali. We also obtain an alkaline solution of oxide of chrome on taking it recently precipitated, diluting it with a little water, and dissolving in this water some pieces of caustic potash; on afterwards diluting the combination with water and filtering, we obtain a liquor of a fine green, which, on ebullition, deposits the oxide it contains, and the liquor remains colourless.

Chromate of Potash.

There are two kinds of chromate of potash; the one neutral is of a citron yellow, and crystallizes in small prisms. This salt, on the addition of heat, assumes a fine red, but returns to its natural colour when cooled. The second has an excess of acid; its colour is orange-red, and it crystallizes in beautiful prisms of the same colour.

Chromate of Ammonia.

When we saturate ammonia by the chromic acid, and abandon the liquor to a spontaneous evaporation, an arborescent salt is formed out of the liquor, composed of tufts of fine yellow: sometimes it is presented in the form of pearl-

pearl-like laminæ. To conclude, this salt is easily decomposed by heat; even when it is dissolved, brown flakes are separated, which are oxide of chrome, and which become green by calcination.

Chromate of Lime.

Chromic acid forms a very soluble salt with lime; its solution furnishes by evaporation silky flakes of a yellowish brown, which are easily dissolved in water: this salt is decomposed by the fixed alkalis.

Chromate of Magnesia.

Magnesia easily combines with chromic acid: the salt which results from it is very soluble in water; the solution crystallizes in prisms with six faces perfectly transparent, and of a fine topaz yellow: when they are in considerable quantity the colour is orange yellow.

Magnesia is separated from the chromic acid by the fixed caustic alkalis and the alkaline earths.

Metallic Chromates.

If into a solution of sulphate of iron at the *minimum* we pour chromate of potash, we obtain a fawn-coloured precipitate, which, when treated with caustic alkali, gives no trace of chromic acid: this precipitate is dissolved with great facility in muriatic acid, from which the alkali separates it completely without the least trace of alkaline chromate remaining. The nitric acid dissolves a part of the precipitate, and assumes a fine green colour: this precipitate therefore is not a chromate of iron, but a mixture or combination of oxide of iron and oxide of chrome, which seems to resemble strongly what is presented to us by nature.

It is evident from the result of this experiment, that the chromic acid has been decomposed by the oxide of iron, which, in passing to the *maximum*, has reduced the other to the *minimum*, or to the state of green oxide. If we wished therefore to form chromate of iron, it would be necessary to employ this last metal saturated with oxygen, in order that it may not be able to act upon that of the chrome.

Chromate of Lead.

This combination assumes different shades, according to the manner in which it has been prepared.

If the chromate of potash be neutral, we obtain an orange-yellow colour; if it has an excess of acid, the colour

lour is deep citron yellow: if the alkali on the contrary be predominant, the shade is a reddish yellow, and sometimes a fine deep red: the shades also vary in proportion as we operate in the hot or cold manner.

Chromate of lead made with a solution slightly acid is that which is most in request by painters, and it is in fact the most solid. We may heighten its colour, either by a little alkali, or by precipitating it hot with acetate of lead. In the latter case it should seem that a part of the acetic acid is separated, and that the oxide of lead which it abandons is united to the common chromate and heightens its colour.

We should conceive that the chromates which contain an excess of oxide of lead must be more alterable by sulphurous vapours than those in which this oxide is saturated by the chromic acid.

Chromate of Copper.

The simplest way of forming chromate of copper is to mix a solution of neutral chromate of potash with a solution of sulphate of copper: a brownish-yellow precipitate is formed, which, when well washed and dried, assumes a bistre brown colour.

Chromate of Silver.

Chromate of silver is prepared by decomposing nitrate of silver by neutral chromate of potash: a reddish brown precipitate is produced when the operation is performed with heat, and of a purple-red when done in the cold: lastly, it is of a carmine red colour if the solution of chromate of potash contains a slight excess of acid: in the latter case the precipitate is not so quickly formed, and is less abundant, but it is crystallized in small semitransparent grains.

This salt becomes brown on exposure to light; it is soluble in the nitric acid, and from which the muriatic acid separates the oxide of silver.

Uses to which the Preparations of Chrome are applied.

This oxide is now generally employed in the porcelain manufactories throughout France. It supports better than any other metal, and without undergoing any alteration, the intense heat employed to prepare hard porcelain: it produces an extremely beautiful green, which has not yet been obtained with any of the other metals.

A very fine enamel resembling in colour the emerald of Peru is made with the oxide of chrome. Another enamel

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is also made with it, which, when applied upon copper or silver, furnishes a colour precisely similar to that of fine gold, and imitates this bright metal extremely well when applied in thin leaves to other metals: a colour which, I think, cannot be obtained in the same degree of perfection with any other metal.

I shall not dwell upon the different varieties of chromates of lead used in painting: they are already well known to artists, and are in great request on account of their beautiful colours, the facility with which they may be applied, and their great inalterability.

It is very probable that several other metallic chromates would also furnish beautiful colours if they were properly examined by painters.

IV. *Observations on loaded and unloaded Barges, Boats, Beams, or floating Bodies descending with Streams or Currents, and why the heavier End will go foremost. By* George Orr, *Esq.*

In addition to what I have already said on this subject, I now submit what follows to the consideration of scientific men: and as my object is an endeavour to attain the truth on so interesting a subject, I am ever ready to admit my errors where I am wrong, but hope that reason and good temper, free from peevishness and personality, will ever regulate the discussion of philosophical subjects.

When bodies of the description alluded to, or in fact any bodies that are specifically lighter than water, float in it, there are two powers always opposed to each other, that is, the specific gravities of the fluid and of the floating body; and in proportion as these differ more or less, in the same proportion will the two bodies oppose each other: for the less specifically heavy any floating body is, the less power will it possess to contend against the fluid in its endeavours to sink or descend; and of course it will follow that it will float nearer the surface. When water is perfectly at rest, it has found its level, and its surface presents a horizontal plane; or in other words, all its particles press or gravitate towards the centre of the earth in perpendicular and right lines; but on any change from this state of rest taking place, the particles of water are, by the force of gravitation, put in motion, and will endeavour to find their level again, or continue to move on an inclined plane; and all bodies suspended in the fluid, or floating on the surface more or less deep, being subject

to the same laws of gravitation, take their direction with the moving fluid, and thus pass down the same inclined plane, with a motion more or less accelerated as such bodies are heavier or lighter; that is, as they possess a greater or less power to overcome the resistance that may be opposed to them.

I think it is manifest from what is here said, that the motion which takes place with regard to both fluid and solid, is owing to the attraction of gravitation; and as the velocities of bodies arising from this power are greatest in descending the perpendicular to the plane of the horizon, so it will follow that these velocities will be diminished until the line, along which they may descend, being carried from the perpendicular round the whole quadrant or right angle, arrives at the level or parallel to the horizon, where, if the power of descending be totally opposed, or the centre of gravity altogether supported, no motion will take place. Hence it will follow that the velocity of a stream, river, or current, and of course of bodies that float in them, will be greater or less, as the inclination of the plane on which they descend departs more or less from the line bounding the horizontal plane.

Loaded barges, beams of heavy wood, &c., without towing, and that float with the tide, will make a quicker progress than the tide; the same will take place in a river or stream where there is no tide. Captain Burney having asked the bargemen on the Thames the reason of this, their reply was, "That loaded barges had more hold of the tide from their floating deeper than unloaded ones." This was but a bad way of accounting for it; but some reason must be assigned by those who continually observed this, and who were ignorant of the real cause of it.

The reason of this quicker progress seems to me to be this: any solid floating in a fluid, and descending with it, acts altogether in one mass; and its particles thus acting together conspire to overcome the resistance they meet, and to divide the fluid, which, easily yielding to any pressure, will make way for the body in its descent pressing forward. Besides, the particles of the fluid do not act in conjunction, and being easily separated they will roll about and impede each other by their friction, not only against each other, but also against the sides of the river, and the bed on which they descend. To this may be added, that less friction takes place between the fluid and solid, as they attract each other less, than between the innumerable particles of the fluid; consequently the solid will glide on, or slip through the

liquid

liquid body with a greater degree of velocity than the fluid, under the circumstances already enumerated, can attain.

Captain B. says, this greater progressive motion in the floating body is not owing to a more rapid under-current. I think not, but to the causes already assigned. Captain B. says, the surface of the ocean is an inclined plane. I have stated in some letters several months ago in the Philosophical Magazine, that the surface of the ocean, owing to the attraction of gravitation and its laws, and to the impression of the winds, &c., consists of an infinity of inclined planes, or ascents and descents.

No collision could take place between a heavier and lighter barge or body passing London bridge with the ebb of the tide, let them be ever so near, provided the heavier body were foremost, because on its descent it will, from its greater weight, acquire a *momentum* which will carry it on more rapidly than the lighter body; but if the lighter body were foremost and very near, it might be overtaken, and a collision take place.

When the wind blows strong into any bay, or against an embayed coast, there must be an under-current, because the wind prevents the return of the accumulated water along the surface. All pressure on bodies floating with streams must, whether the pressure be perpendicular or oblique, increase their progress:—if the pressure be perpendicular, it adds to the weight, and consequently to their power of overcoming resistance on the part of the fluid: if the pressure be oblique, and in direction of the motion, it will, besides increasing the weight, give impulse.

The reason why ships at sea that are deeply loaded make less progress on a voyage than those that are lighter, and which seems to be in contradiction to what was last stated, seems to me to arise from the following cause: that is, that what they gain by their gravity over a lighter vessel in descending from the top of the wave or inclined plane, they lose in ascending the next; for it is manifest that the surface of the ocean consists of innumerable inclined planes, or ascents and descents; but in a river or running stream the whole progress is on a descent.

Two pieces of wood of the same kind and of the same weight, but of different shapes, the one, for instance, a cylinder with all its transverse diameters equal, the other of a conical form, with its transverse diameters or lines all unequal, would, in my opinion, differ considerably in their progress in the same fluid, the cone making greater way than the cylinder; and to this I presume it may be principally at-

 tributed

tributed why one ship sails better than another, and which, on some future occasion, I may attempt to demonstrate.

Timber used at sea to find the direction of any current ought to be in its shape conical, and a good deal so, but the base of the cone to be rounded, or the sharp angles taken off, in order that it may pass more fairly through the water; for wood of this shape will take the direction better, and be more easily observed: in such cases there should always be two conical timbers, and if they both take the same direction, the tendency of the current is certainly proved. No experiments can be made on this subject with South Sea clubs, such as captain B. used, nor with anything that will sink in water, that is, with any body specifically heavier than water.

Supposing a barge loaded at one end and empty at the other, and without a helm, if it floated in a fair and regular stream without currents, it would certainly proceed with the heavy end foremost, for the same reasons that a conical piece of wood, or even a cylindrical one loaded at one end, would go with their heavy end foremost in a fluid, though originally placed in a contrary direction: it would be the same with the cone and cylinder, if placed across a dry inclined plane; that end containing the greatest quantity of matter, from its power of overcoming resistance, would always have a tendency to be foremost. The inclined plane of rivers and streams must, from the number of inequalities at bottom, be a very irregular one—the current will partake of those irregularities, and the motion of bodies that float in them must be affected by them in a certain degree.

Lastly, Does a body floating down a stream or current, and which has a quicker progress than the stream or current, receive any addition to its motion from the motion of the fluid? I think it does not; that it is only indebted to the fluid for its suspension, but that it is to its own gravity, and acting in one mass, that it is indebted for its greater progress. On the contrary, I think it loses in its velocity or progressive motion; for though the floating body be specifically lighter than the water, and of course one would imagine that it would move slower, still, owing to the causes already enumerated, its progress is quicker: but there is a drawback on this progress; because, if the solid overtake the water, part of its force must be wasted against that body which moves slower, but in the same direction. This case is analogous to that of two balls moving

in

in the same direction with different velocities; that with the greater velocity, on overtaking the other, communicates part of its force, and of course loses so much in its velocity.

V. *On Nævi Materni.*

To Mr. Tilloch.

Sir, The last Number of your Philosophical Magazine contains a discussion, by one of your ingenious correspondents, in support of the old prejudice that certain congenital marks or excrescences, commonly called *nævi materni*, originate from the influence of the mind of the parent. As the paper, no doubt, was written with a serious object, I comply with the writer's request in forwarding to you for publication some observations on the subject.

It is agreed that *nævi materni* resemble known objects in nothing more than *form*; although, as Enceps (the writer's signature) observes, "more than one volume has been filled with reputed instances of the effects of the mother's imagination upon her offspring." The *forms*, however, of *nævi materni* in general are so far from having a resemblance to any known object, that the experience of practitioners, who are in the daily habits of operating on some, and seeing many others, proves as much as extensive experience can prove, that the relations of *nævi materni* resembling a bunch of grapes, or a bit of bacon, are to be classed with the idle tales of the nursery. Even Enceps himself "was for a long time prepossessed with the same notion," viz. that such fanciful forms of *nævi materni* were "idle tales," till a case "was related to him by an intelligent friend who had seen a child born with only one leg, as well as its mother, who declared her firm belief that the cause of this imperfection in her child was a violent fright which she experienced from seeing a beggar suddenly uncover the wounded stump of his thigh." This, however, is not an example of *nævus maternus*, but of *monstrosity*. Now I will seriously ask Enceps, at what period of pregnancy this unfortunate mother received such a *violent fright*. In the latter months of pregnancy, *after* the limbs of the fœtus had been formed? The separation of a limb, and its still more extraordinary annihilation, must then be explained as the effects of imagination,—*or* in the early months of pregnancy, *before* the ovum had become organized!

ized! The imagination must then be supposed to act on a part of the fœtus before such a part had any existence. The common opinion is, that monstrosity depends on *original conformation* of the ovum; nor is it by any means intelligible that a metamorphosis or annihilation of any part of the fœtus can take place from an impression on the imagination of the parent.

I have said that Enceps's object in writing was not levity; but when I came to the *tale* of the cat and the kittens, my suspicions were somewhat roused. "A pregnant *she* cat" had its tail trodden upon, and lo! when she littered she had an *even* number of kittens; otherwise the surprising things that did happen, could not have happened—that "*half* of her kittens had their tails bent in the middle!" and that *half* had not. The circumstance which roused my suspicion was the gravity with which Enceps says "this fact (or rather this *tale*) seems to be very important, and *to prove nearly to a demonstration*, that the imagination of pregnant females has the power of acting on the bodily conformation of their young!!"

As a medical man, I had reason to hope that the old prejudice of the perfection of the fœtus being in anywise contingent on the imagination of the parent was at an end; and having long been a reader of your respectable Magazine, I could not observe Enceps's reasoning in favour of such a prejudice, without presuming that it was subject to animadversion.

I remain, sir, your humble servant,

CHIRURGICUS.

December, 1809.

VI. *Memoir on the Mineralogical Geography of the Environs of Paris. By Messrs.* CUVIER *and* BROGNIART*.

THE country in which Paris is situated is perhaps one of the most remarkable hitherto observed, from the succession of the various soils which compose it, and from the extraordinary remains of ancient organizations which it contains: myriads of sea shells, regularly alternated with fresh-water shells, form the principal mass: bones of terrestrial animals, entirely unknown even with respect to their genera, fill certain parts: other bones of species remarkable from their size, and the counterparts of which are only

* *Annales du Muséum d'Histoire Naturelle*, tome xi. p. 293.

found

found in very distant countries, are scattered in the strata nearest the surface: a strongly-marked character of a great eruption proceeding from the south-east is imprinted on the forms of the eminences and the directions of the valleys: in short, there is no district better adapted for making us acquainted with the final revolutions which put a termination to the formation of our continent.

This country has nevertheless been very little studied under this point of view; and although so long inhabited by many enlightened men, whatever has been written on the subject has been confined to some detached fragments, and almost all of them are either exclusively mineralogical, without any regard to organized fossils, or purely geological, without reference to the position of these fossils.

A memoir of Lamanon, on the gypsums and their fossil bones, may perhaps form the only exception to this classification: we are bound, however, to acknowledge that the excellent description of Montmartre by M. Desmarets, the information given by the same author as to the basin of the Seine, in the *Encyclopédie Méthodique*, the mineralogical essay on the department of Paris by M. Gillet-Laumont, the extensive researches on the fossil shells of the environs of Paris by M. Lamarck, and the geological description of the same district by M. Coupé, have been advantageously consulted, and have several times directed our steps.

We presume to think that the task of which we are about to present the class with a sketch, will not be without interest, notwithstanding the works above alluded to.

Four years ago we commenced our labours; and although we have persevered in them by making numerous excursions, collecting specimens and information from every quarter, we are far from thinking we have done enough, and we earnestly desire that our readers may not confound the abridgement which we are about to give, with the full details which we propose to publish. Some circumstances compel us to present this abridgement at this moment, and to assign a date * to such tedious and laborious researches, before the happy period at which we shall think them brought to a conclusion.

From the nature of their object, our sketches were limited according to the nature of the soil, and not according to arbitrary divisions.

We thought it right therefore, in the first place, to de-

* This Memoir was printed in Jan. 1809.—Edit.

termine the physical boundaries of the district which it was our object to study.

The basin of the Seine is separated for a long space from that of the Loire by an extensive high plain, the greater part of which vulgarly bears the name of Beauce, and the middle and driest part of which extends from the north-west to the south-east, over an extent of more than forty leagues, from Courville to Montargis.

This plain is bounded towards the north-west by a higher, and in particular a more broken district, from which the rivers Eure, Aure, Ilon, Rille, Orne, Mayenne, Sarte, Huine, and Loire, arise. The highest part of this district is between Sees and Mortagnes, and which formerly composed the province of Perche, and a part of Basse-Normandie, and which now belongs to the department of the Orne.

The line of separation between Beauce and Perche passes close by the towns of Bonnevalle, Alluye, Iliers, Courville, Pontgouin, and Verneuil.

On all other sides the plain of Beauce overlooks every surrounding district.

Its slope towards the Loire is not interesting to our subject.

The slope towards the Seine is divided into two inclinations, one of which on the west looks towards the Eure, and the other on the east looks towards the Seine.

The first proceeds from Dreux towards Mantes.

The other begins from the neighbourhood of Mantes, passes by Marly, Meudon, Palaiseau, Marcoussy, the Ferté-Alais, Fontainebleau, Nemours, &c.

But it must not be concluded that these two inclined planes are straight or uniform; on the contrary, they are in all directions unequal and rugged, to such a degree that, if this vast plain were surrounded with water, its edges would furnish gulfs, capes, and straits, and would be every where surrounded by small islands.

In the same manner in our environs, the long mountain on which are situated the woods of St. Cloud, Ville-d'Avray, Marly, and Aluets, and which extends from St. Cloud to the confluence of the river Maulde in the Seine, would form an island separated from the rest by the strait in which Versailles is situated, the little valley of Sevres, and the great valley containing the park of Versailles.

The other mountain in the form of a fig leaf, on which are situated Bellevue, Meudon, the woods of Verriere, and those of Chaville, would form a second island separated from

from the continent by the valley of Bievre and that of the hills of Jouy.

But afterwards, from St. Cyr to Orleans, there is no longer any complete interruption, although the rivers Bievre, Ivette, Orge, Etampes, Essonne, and Loing, cut deeply into the continent on the east coast, while the rivers of Vesgre, Voise, and Eure, do the same towards the west.

The most rugged and uneven part of the surface, and that which would furnish most islands, would be what is vulgarly called the *Gâtinois François;* and particularly that part in which the forest of Fontainebleau is situated.

The slopes of this immense platform are generally very abrupt; and all the ravines which we find in them, as well as those of the valleys, and the wells dug in the high parts, show that its physical nature is the same every where, being formed of one prodigious mass of fine sand which covers the whole surface, passing equally over all the other soils or inferior platforms which this great plain overlooks.

The edge of this platform towards the Seine, from the Maulde to Nemours, will therefore form the natural limit of the basin which we are about to examine.

From below its two extremities, *i. e.* towards the Maulde and a little beyond Nemours, immediately issue two portions of a platform of chalk, which extends in every direction and to a great distance, in order to form the whole of Haute Normandie, Picardy, and Champagne.

The interior edges of this great girdle, which pass from the east by Montereau, Sezanne, and Epernay; from the west by Montfort, Mantes, Gisors, and Chaumont, in order to approach Compiegne, and which form at the north-east a considerable re-entering angle which embraces the whole of the Láonnois, complete, together with the sandy coast now described, the natural limit of our basin.

But there is this great difference, that the sandy platform which comes from Beauce is higher than the others, and is consequently the most modern, and finishes completely the stretch of coast which we have marked; while, on the contrary, the platform of chalk is naturally more ancient and lower than the rest, only ceasing to appear outside of the girdle above mentioned; but so far from being at an end, it visibly sinks under all the other strata: we find it in short wherever we dig sufficiently deep under the latter, and it even rises up in some places, piercing as it were through the other strata.

We may therefore conclude that the materials which compose the basin of Paris, in the directions to which our

inquiries were limited, have been deposited in a vast hollow or gulf, the bottom of which was of chalk.

This gulf perhaps formed a complete circle, or a kind of great lake; but we cannot ascertain this, in consequence of its edges on the south-west having been covered, as well as the materials of which they were composed, by the great sandy platform first mentioned.

We may add that this great sandy platform is not the only one which has covered the chalk; there are several in Champagne and Picardy, which, although smaller, are of a similar nature, and may have been formed at the same time. Like it, they are placed immediately over the chalk, in the places where the latter was so high as not to admit of its being covered with the materials of the basin of Paris.

We shall in the first place describe the chalk, the most ancient of the substances which we have in our environs, and conclude with the sandy platform, the most recent of our geological productions.

In the intermedium between these two extremes we shall speak of less voluminous but more varied substances, which had covered the great cavity of the chalk before the platform of sand was deposited on some of them.

These substances may be divided into two soils (*étages*).

The first (which covers the chalk wherever it was not sufficiently high, and which has filled the a hole of the bottom of the gulf,) is itself subdivided into two parts of equal level, and placed not upon one another, but end to end: viz.

The platform of siliceous lime containing no shells.

The platform of lime with coarse shells.

We are sufficiently well acquainted with the limits of this soil on the chalky side, because the chalk does not cover it; but these limits are marked in several places by the second soil, and by the great sandy platform which forms the third, and which covers a great part of the two others.

The second soil will be named *gypso-marley*.

It is not generally spread, but merely scattered from space to space, and as it were by spots; these spots also are very different from each other in thickness, and in the details of their composition.

These two intermediate soils as well as the two extreme soils are covered, and all the vacuities which they have left are partly filled, by a fifth sort of soil, mixed also with marle and silex, and which we call *fresh-water soil*, because it abounds in fresh-water shells only.

We

We have the honour to present the class with the first of a series of mineralogical charts, in which each kind of soil is coloured differently.

The sand is fawn-coloured; gypsum blue; shelly lime yellow; siliceous lime violet; chalk red colour; fresh-water soil green streaked with white. We have here marked in plain green, the worn or alluvial sands which have not been tranquilly deposited, but brought from other quarters by currents; and in dark brown the peaty soils formed along the rivulets and round the pools of water.

This chart, one of the principal results of our travels, is complete in the coloured part, and we have only left uncoloured that with which we are not yet sufficiently acquainted.

Such are the great masses of which our district is composed, and which form the different strata. But on subdividing each stratum we may attain still greater precision, and obtain still more rigorous mineralogical determinations, which will give so many as ten distinct kinds of strata, of which we shall now present a rapid enumeration.

ARTICLE I. *Formation of Chalk.*

Chalk forms in the environs of Paris, as in almost all those places where it has been observed, a mass in which the strata are frequently so indistinct that we are almost inclined to doubt whether it was formed by beds, if we did not see these beds interrupted by silex, which, by their perfectly horizontal position, their parallelism, their continuities, and their frequency, indicate successive and almost periodical depositions.

Their respective distance varies according to the place: at Meudon they are about two metres (78¾ Eng. inches) from each other, and the space comprehended between any two beds of silex does not contain any detached pieces of this stone. At Bougival the beds are divided, and the silex is much less abundant.

The chalk which contains flints is not pure carbonated lime: it contains, according to M. Bouillon Lagrange, about 0·11 of magnesia and 0·19 of silex, the greatest part of which is in the state of sand, which we may separate by washing. The fossils found in it are not numerous, in comparison with those which we observe in the strata of coarse limestone (which almost immediately cover the chalk), but they are entirely different from these fossils, not only in the species, but even in the genera.

On

On adding those which we observed ourselves, to those which have been collected by M. Defrance, we extend to the number of fifty the various species of fossils with which we are acquainted in the chalk, of the soils which are the objects of our study.

All the species of these fossils have not been as yet determined, and we shall give in our subsequent detailed Memoirs their precise enumeration and determination: we shall content ourselves with saying on the present occasion, that we have found

Two *lituolites*.

Three vermiculites.

Some belemnites, which, according to M. Defrance, are different from those which accompany the ammonites of compact lime.

Some fragments of shells, which, from their tubular form and fibrous structure, cannot be referred to any other than the genus *pinna:* but if we were to infer from the thickness of these fragments the size of the individuals to which they must have belonged, we must conclude that these testacea must have been monstrous. We measured pieces 12 millimetres (·47 inches) thick, while the thickness of the largest kinds of pinna known is only two millimetres (·08 inches).

One muscle shell.

Two oyster ditto.

A species of the *pinna* genus.

A cranium.

Three anomites (*térébratules*).

One *spirorbis*.

Some *ananchites*, the crustaceous envelope of which has remained calcareous and assumed the sparry texture, while the middle part only is changed into silex.

Some *porpytes*.

Five or six different *polypiers:* one of them seems to belong to the genus *caryophyllæa*, and another to the genus *millepora*. This last is generally brown, and in the state of oxidated iron, resulting from the decomposition of pyrites.

Lastly, some teeth of squali.

We shall observe with M. Defrance, that there has not been found in chalk any univalve shell with a simple and regular spire. This fact is the more remarkable, as we meet with these shells in great abundance, some metres (39·33 inches) above the chalk, in calcareous strata also, but of a different structure.

Among the quarries and hills of chalk which we have visited,

visited, we shall mention Meudon. The chalk is here covered by plastic clay and coarse limestone. The upper part of this mass is as it were broken, and presents a kind of rubble (*brèche*), the fragments of which are chalk, and the intervals clay.

The highest part of the mass of chalk appeared to us to be above the glasswork of Sevres. It is 15 metres ($49\frac{1}{4}$ feet) above the Seine. This disposition raises up all the strata of soil which surmount it, and seems at the same time to diminish their thickness. The mass of stone is sensibly inclined towards the banks of the river.

At Bougival, near Marly, the chalk is almost entirely exposed in some places, being only covered by calcareous stones of a very fine grain, but in fragments more or less large, and scattered in a marley sand, which is almost pure at the top.

Amidst these fragments we find geodites, of a yellowish-white limestone, compact and fine grained, with sparry laminæ, and small cavities strewed with very small crystals of carbonated lime. The paste of these geodites contains a multitude of small spiral univalve shells; which seems to prove that this limestone does not belong to the chalk formation.

Among these geodites we found one, which presented a large cavity fringed with long and acute limpid crystals, upwards of two centimetres (·79 inches) long.

Mechanical division alone informed us, that these crystals belonged to the species of sulphated strontian, and a more attentive examination of their form instructed us that they constituted a new variety. M. Hauy, to whom we communicated the circumstance, called it *apotamous sulphated strontian.*

These crystals present rhomboidal prisms with four panes, the angles of which are the same with those of the prism of the unitary, or blunted (*emoussée*) varieties, &c., *i. e.* 77° 2′ and 102° 58′. They are terminated by pyramids with four faces and very acute. The angle of incidence of the faces of this pyramid on the adjacent panes is 161° 16′. The faces are produced by a decrement by two ranges to the left and right of the angle E of the subtractive molecule. This is a law which had not hitherto been recognized in the varieties of sulphated strontian examined, to this day.

Its sign will be $\overset{1}{E}\,E^2\,{}^2E$.

The crystals of strontian hitherto observed in the environs of Paris are extremely small, and fringe the sides of some

some of the geodites of strontian found in the green marles of gypsous formation; but we have not as yet seen any of them so large and so well defined.

Article II. *Formation of Plastic Argil.*

Almost the whole surface of the mass of chalk is covered with a stratum of plastic clay which has some very remarkable common characters, although it presents sensible differences in various points.

This clay is unctuous, to the touch tenacious, and contains silex, but very little lime; so that it does not effervesce with the acids. It is even absolutely infusible in a porcelain furnace when it does not contain too great a proportion of iron.

It varies considerably in colour:—at Moret in the forest of Dreux, it is very white: at Montereau, Houdan, and Condé, it is gray: pure slate gray, slate gray mixed with red, and almost pure red, throughout the whole south of Paris, from Gentilly to Meudon.

This plastic clay is, according to its various qualities, employed in making porcelain, stone-ware, crucibles, or common red earthenware. It is never effervescent nor fusible. The red colour, the pyritous grains, the portions of silex, the small fragments of chalk, and the crystals of selenite which it sometimes contains, are the only defects found in it.

This stratum varies much in thickness: in some parts it is 16 metres (52½ feet) and upwards; in others it forms only a thin covering of one or two decimetres (4 or 8 inches).

It seems almost certain, that no marine nor terrestrial fossil is found in this clay, at least we have not seen any, either in the different strata we examined in their respective positions, or on the large heaps which we repeatedly examined, in the numerous manufactories in which it is used; besides which, the workmen who dig up this article in the south of Paris assured us, that they never found in it any shells, bones, wood, or vegetables.

Dolomieu, who discovered this same bed of clay between the chalk and the coarse limestone in the elbow formed by the Seine in front of Rolleboise, says indeed that fragments of bituminous wood have been found in it, and that they have been even taken for coal; but he remarks, that these small woody portions have been found in pieces rolled down from the bank, which may have enveloped them at a period posterior to the primitive deposit of this clay.

The

The places mentioned above prove, that this stratum of clay is very extensive, and preserves throughout its whole extent its principal characters of formation and position.

If we compare the descriptions we have given of the beds of chalk and beds of plastic clay, we shall remark: 1st, That not only we do not find in the clay any of the fossils met with in the chalk, but we do not even find any fossil in it. 2d, That there is no insensible passage between the chalk and the clay, since the parts of the bed of clay most adjacent to the chalk do not contain lime more than the other parts.

It appears to us that we may conclude from these observations, in the first place, that the liquid which has deposited the bed of plastic clay was very different from that which deposited the chalk, since it does not contain carbonated lime in any sensible quantity, and since none of the animals live there which inhabited the waters that deposited the chalk. Secondly, that there must necessarily have been a distinctly-marked separation, and perhaps even a long period of time between the deposit of the chalk and that of the clay, since there is no transition between these two kinds of soil. The kind of broken fragments of chalk and clay which we remarked at Meudon * even seems to prove that the chalk was solid when the clay was deposited. This earth is insinuated between the fragments of chalk produced on the surface of the chalky soil by the motion of the waters, or by some other cause.

The two kinds of soil which we have described have been produced, therefore, under completely different and even well-defined circumstances. They are the results of the most distinct and best characterized formations which can be found in geology, since they differ in their chemical nature, in the kind of stratification, and above all in the kind of fossils which we meet with in them.

Article III. *Formation of Sand and of coarse Limestone.*

The coarse limestone does not always immediately cover the clay, being frequently separated by a stratum of sand of various thickness. We cannot say whether this sand belongs to the formation of the calcareous earth, or to that of the clay. We have not found shells in it, in the few places where we have observed it, which would give it an

* See page 43.

argillaceous

argillaceous formation; but the lowest calcareous stratum being generally sandy and always filled with shells, we do not as yet know if this sand be different from the first, or if it be the same formation. What would lead us to suppose that it was different is, that the sand next the clays which we have seen, is generally very pure, although of a red or bluеish gray colour; it is refractory, and frequently in very coarse grains.

The calcareous formation reckoned from this sand is composed of alternate strata of coarse limestone, more or less hard, of argillaceous marle, and even leafy clay in very thin strata, and of calcareous marle; but it must not be supposed that these various strata are placed there by chance, and without following some general law: they uniformly follow the same order of superposition in the considerable extent of soil which we have examined. In some places several of them are entirely wanting, or are very thin; but that which was the lowest in one district never becomes the uppermost in any other.

This constancy in the order of superposition of the thinnest strata, and over an extent of 12 myriametres (74½ miles) at least, is in our opinion one of the most remarkable facts which we have established. From this there ought to result most interesting consequences to the arts and to geology.

The means which we have employed for ascertaining, in the midst of so great a number of calcareous beds, a stratum already observed in a very distant district, is taken from the nature of the fossils contained in each stratum, these fossils being generally the same in the corresponding strata, and present differences of species sufficiently remarkable between one system of strata and the other. This is a mark which has not as yet deceived us.

It must not be thought, however, that the difference between one stratum and another is equally well marked with that between the chalk and the limestone. If this was the case, we should have an equal number of distinct formations; but the characteristic fossils of one stratum become less numerous in the next superior stratum, and disappear completely in the rest, or are replaced gradually by new fossils which had not as yet appeared.

We now proceed to point out (following the same course) the principal systems of strata which may be observed in the coarse limestone. In the detailed Memoir about to be published will be found the complete description, stratum by stratum, of the numerous quarries which we have observed in order to procure materials for our publication.

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The lowermost strata of calcareous formation are the best characterized: they are very sandy and frequently even, rather sandy than calcareous. When they are solid, they are decomposed and fall into dust on the first exposure: this kind of stone therefore is not fit to be used.

The shelly limestone which composes it, and even the sand which sometimes supplies its place, almost always contain green earth in powder or in small grains. This earth, from our experiments, appears to be analogous to *terra veronica*. It owes its colour to iron; it is found in the lower strata only: we do not find it in the chalk, in the clay, nor in the middle or upper calcareous strata, and we may regard its presence as a certain indication of the proximity of plastic clay, and consequently of chalk. But what characterizes still more particularly this system of strata, is the prodigious quantity of fossil shells which it contains. In order to give an idea of the number of species which these strata contain, it will be sufficient to say that M. Defrance has found more than six hundred species, all of which have been described by M. Lamarck.

We have to remark that most of the shells of the lowest of these strata are much further removed from the present existing species, than those of the upper strata. We shall mention among the fossils peculiar to these lower strata, periwinkles, oysters, muscles, pinnæ, calyptreæ, pyrulæ, large tellines, terebellæ, porpytes, madrepores, and particularly nummulites and fungites.

Such are the names of the most remarkable shells in this stratum: we ought to remark that it was not in the depôt of Grignon alone that we gathered the specimens we have described; such examples would not have characterized the system of strata which we wish to make our readers acquainted with: we chose them from the quarries of Sevres, Meudon, Issy, Vaugirard, Gentilly; in the strata of Guespelle; in those of Pallery near Chaumont, &c.

It is in this same stratum that we find the camerines. They are either by themselves or mixed with madrepores and the preceding shells. They are always the lowest, and consequently the first which are deposited on the chalky formation; but this is not the case every where. We have found some of them near Villers-Cotteret, in the valley of Vaucienne; and at Chantilly on the declivity of the mountain. They are mixed here with shells in good preservation, and with coarse grains of quartz, which, together, make it a kind of puddingstone, on Mount Ganelon

helon near Compiegne, and Mount Ouin near Gisors, &c.

Another character peculiar to the shells of this stratum is, that they are mostly very entire and well preserved, being easily detached from their rock, and some of them have preserved their pearly state. In all the preceding places, and in others, this is less remarkable; because we ascertained that the sandy calcareous strata, which contain these shells, immediately follow the plastic clay which covers the chalk; and it is by these multiplied observations that we recognized the generality of the scale which we have laid down.

The other systems of strata are less distinct, and we have not as yet been able to arrange the numerous observations which we have made, in order to establish with precision the succession of the different fossils which ought to characterize them. We can announce, however, that, from our inspection of the quarries to the south and west of Paris, from Gentilly to Villepreux and Saint-Germain, the strata above those which we have described succeed each other in the following order.

1. A soft stratum frequently of a greenish colour, this is called the *green bank* by the workmen. It frequently exhibits on its lower surface brownish marks of leaves and stalks of vegetables.

2. Gray or yellowish strata, sometimes soft, sometimes very hard, and containing chiefly roundish venuses, camreys, and particularly tuberculated cerites, which last are sometimes in prodigious quantity. The upper and middle part of this stratum, frequently hard, is a very good stone building, and is known by the name of *the rock*.

3. Lastly, and towards the upper part, a stratum, not thick but hard, which is remarkable from the prodigious quantity of small long and striated tellines which it presents in its seams. These tellines are laid horizontally, and closely wedged against each other. They are generally white.

Above these last strata of coarse limestone we find hard calcareous marles divided into fragments, the faces of which are generally covered with a yellow coating and with black dendrites. These marles are separated by soft calcareous marles, by argillaceous marles, and by calcareous sand, which is sometimes agglutinated, and it contains horny silex with horizontal zones. We refer to this system the stratum of the quarries of Neuilly, in which we find crystals of quartz and rhomboidal crystals of variegated carbonate of lime.

But

But what characterizes more particularly this last system of strata of calcareous formation, is the absence of every shell and of every other fossil.

It results from the observations which we have related:

1st, That the fossils of coarse limestone have been deposited slowly and in a calm sea, since these fossils are deposited by regular and distinct strata; that they are not indiscriminately mixed, and that most of them are in a state of perfect preservation, however delicate may be their texture; the points of the prickly shells being very often unbroken.

2dly, That these fossils are entirely different from those of the chalk.

3dly, That in proportion as the strata of this formation were deposited, the number of the species of shells diminished until no more are found. The waters which formed these strata either have not contained any, or have lost the property of preserving them.

There can be no doubt that things went on very differently in these seas from what they do at present in the waters of the present day; where no strata are formed, the species of shells found in them are always the same in the same regions; we do not find, for example, since we began to fish for oysters on the shores of Concale, that this kind of shell-fish has been replaced by other kinds.

Art. IV. *Gypsous Formation.*

The soil which we are now about to describe, is one of the clearest examples of what is meant by the word formation. Here we find strata very different from each other in their chemical nature, but evidently formed together.

The soil which we call gypsous is not composed of gypsum alone, it consists of alternate strata of gypsum and of argillaceous and calcareous marle. These strata have pursued an order of superposition, which has been always the same, in the great gypsous girdle which we have studied, and which extends from Meaux to Triel and Grisy. Some strata are wanting in certain districts; but those which remain are always in the same respective position.

The gypsum is placed immediately above the limestone, and it is impossible to doubt this superposition. The position of the gypsum quarries of Clamart, Meudon, and Ville-d'Avray, is above coarse limestone wrought in the same places; that of the quarries of the mountain of Triel, the superposition of which is still more evident; lastly, a well

dug in the garden of M. Lopez at Fontenay-aux-Roses, which first passed through the gypsum, and afterwards the limestone, are ample proofs of the position of the gypsum on the limestone.

The gypsous hillocks have a peculiar appearance, which renders them conspicuous at a great distance: as they are always placed upon the limestone, they form on the higher eminence a kind of second conical or elongated hillock, always distinctly marked.

We shall exhibit the details of this formation, by taking, as an example, the mountain which presents the most complete collection of strata; and although Montmartre has been already well described, it is still the best and the most interesting example that can be selected.

We have recognized, both at Montmartre and on the hillocks which seem to form the continuation of this eminence, three masses of gypsum. The lowermost is composed of alternate and thin strata of gypsum, frequently selenitous, of solid calcareous marles, and of very scaly argillaceous marles. It is in the former that we chiefly see the coarse crystals of lenticular yellowish gypsum, and it is in the latter that we find the menilite silex. We are not acquainted with any fossil in this mass, which is the third, of the quarries.

The second or intermediate mass differs from the foregoing only because the gypsous beds are thicker, and the marley strata are less numerous. We ought to remark that among these masses, that which is argillaceous, compact, and of a marbled gray appearance, serves for building-stone. It is chiefly in this mass, that fossil shell-fish have been seen. No other fossils, however, are found in it; but sulphated strontian has lately been discovered in it, in scattered fragments, in the lower part of the marble-like marle.

The superficial mass, which the workmen call the first, is in every respect the most remarkable, and the most important. It is, besides, much more extensive than the rest, since it is in some places 25 metres (82 feet) thick, interrupted only by a small number of marley strata; and in some places, as at Dammartin and Montmorency, it is situated almost immediately under the vegetable earth.

The lowermost beds of gypsum in this first mass contain silex. The silex and gypsum seem as if mutually dissolved in each other. The intermediate beds are naturally divided into coarse prisms, with several planes, which M. Desmarets has drawn extremely well. They are called the *high pillars*. Finally, the uppermost strata

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are interlaid with marle: they are but thin, and are alternated with strata of marle; there are generally five such, which are continued to great distances.

But these facts, which are already known, are not the most important: we mention them only that they may be brought under view at one glance. The fossils which this mass contains, and those contained in the marle that covers it, present observations of a different interest.

It is in this first mass that we daily find the skeletons of unknown birds and of quadrupeds, which have been already described by one of us (M. Cuvier) in a separate memoir*. To the northward of Paris they are in the gypsous mass itself; here they have preserved their solidity, and are only surrounded by a very thin stratum of calcareous marle; but in the quarries to the southward, they are frequently in the marle which separates the gypsous strata: they have then a great degree of friability. We shall not revert to the manner in which they are situated in the mass; upon their state of preservation, species, &c. these objects having been sufficiently developed in the Memoirs which we have mentioned. We have also found in this mass, bones of tortoises and skeletons of fish.

But what is much more remarkable, and much more important from the consequences that result from it, is, that we find, although very rarely, fresh-water shells. Indeed, one only is sufficient to demonstrate the truth of the opinion of Lamanon and some other naturalists, who think that the gypsums of Montmartre, and of the other hillocks of the basin of Paris, have been crystallized in fresh-water lakes. We shall relate new facts in confirmation of this.

In the last place, the superficial mass is essentially characterized by the presence of the skeletons of mammiferæ. These fossil bones serve to point it out where it occurs in isolated masses; for we have never been able to discover that they have been found in the lower masses.

Above the gypsum are placed strong strata of marle, sometimes calcareous, and sometimes argillaceous.

It is in the lower beds, and in a white and friable calcareous mass, that we have at various times met with trunks of palm-trees converted into silex. They were lying flat, and of a large bulk. It is in this same system of strata that we found (but only at Romainville) shells of the genus *lymnea* and *planorbes*, which seem to differ in no respect from the species now existing in our marshes. One of us has already communicated this important fact to the class.

* Published in *Annales du Muséum.*

It proves that these marles are of fresh-water formation, like the gypsums which they cover.

Above these white marles are also seen very numerous and frequently thick argillaceous or calcareous marles, in which no fossil has been as yet discovered.

We afterwards met with a small bed six decimetres (24 inches) thick, of a scaly yellowish marle, which contains towards its lower part scraps of earthy sulphated strontian, and a little above a thin bed of small elongated tellines, which are lying flat, packed closely into each other. This bed, which seems to be unimportant, is nevertheless remarkable in the first place from its great extent: we have observed it over a space more than ten leagues ($27\frac{1}{2}$ miles) long and more than four (11 miles) broad, always in the same place and of the same thickness. It is so thin, that we ought to know precisely where it lies, in order to find it out. Secondly, because it serves as a limit to the fresh-water formations, and indicates the sudden commencement of a new marine formation.

In fact, all the shells which we meet with above this bed of tellines are also marine.

We find at first, and immediately afterwards, a strong and constant stratum of greenish argillaceous earth, which from its thickness, colour, and continuity, may be recognized at a great distance. It serves as a guide to the tellines, since it is beneath it that we find them. It contains no other fossil, but merely argillo-calcareous geodites and scraps of sulphated strontian. This earth is employed in the manufacture of coarse pottery.

The four or five beds of marle which succeed the green earths are not thick, nor do they seem to contain fossils; but these beds are immediately covered by a stratum of yellow argillaceous marle, which is strewed with fragments of sea shells which belong to the genera cerites, trochi, mactres, venus, cardium, &c. We also meet with fragments of the bones of a thornback.

Almost all the beds of marle which succeed the latter present fossil sea shells, but they are bivalves only; and the last strata (those which are immediately below argillaceous sand) contain two very distinct oyster beds. The first and lowermost is composed of very thick large oyster-shells: some of them exceed a decimetre (4 inches) in length. Afterwards comes a stratum of whitish marle, without shells, then a second very strong oyster bed, but subdivided into several beds. These oyster shells are brown, much smaller and thinner than the above. These last beds of oysters are very frequent, and we have not perhaps seen them twice

fail,

fail, in the numerous hillocks of gypsum which we have examined. The gypsous formation is frequently terminated by a mass, more or less thick, of argillaceous sand, which contains no shells.

Such are the strata which generally compose the gypsous formation. We were at first led to divide it into two, and to separate the history of the marine marles of the top from those of the gypsum, and of the fresh-water marles at the bottom; but the strata are so similar to each other, and accompany each other so constantly, that we have thought it right merely to point out this division without making it in reality.

It now remains to say a few words, as to the principal differences presented by the hillocks which belong to this formation. The gypsous hillocks form a kind of long and broad zone directed from the south-east to the north-east, over a breadth of about six leagues (16½ miles). It seems that in this zone it is only the eminences of the centre which present distinctly the three masses of gypsum. Those of the edges, such as the plaster quarries of Clamart, Bogneux, Antoni, Mont Valerian, Grisy, &c., and those of the extremities, such as the plaster quarries of Chelles and Triel, possess only a single mass. This mass seems to us to be analogous to that which the workmen call the first, *i. e.* the most superficial, since we find in it the fossils of the mammiferæ which characterize it; and since we do not find, in its marles, those coarse and numerous crystals of lenticular gypsum which we observe in the marles of the second and of the third mass.

Sometimes the marles above, are almost entirely wanting; sometimes the gypsum itself is totally wanting, or reduced to a thin bed. In the first case the formation is represented by the green marles, accompanied with strontian. The gypsous formations of the park of Versailles near Saint Cyr, those of Viroflay, are in the former state, and those of Meudon and Ville-d'Avray are in the latter.

We ought to mention here what has been said in another work *, namely, that the gypsous soil of the environs of Paris cannot be referred with accuracy to any of the formations described by M. Werner or his followers. We have on the above occasion assigned our reasons, which it is needless to repeat.

* Brogniart, Minéralogie, tom. i. p. 177.

ART. V. *Formation of Sea-Sand and of Freestone.*

This soil is not extensive, and seems to form a succession to the formation of the marles of gypsum. We should even have brought them together, if it had accompanied them as constantly as the marles accompany the gypsum, and if it had not been frequently separated by a considerable argillaceous mass, stripped of every fossil, and very different in its nature from that which we are now about to describe.

What we have said, shows that this formation generally covers the gypsous formation. It consists in beds of siliceous sand, frequently very pure and agglutinated into freestone, which contains sea shells of various kinds, and all of them of the same kind as those of Grignon. We have here recognized the same oysters, the same calyptreæ, the same tellines, and the same cerites. Sometimes the shells themselves exist, and are in a calcareous state, while in other places nothing remains of them but the external impressions or moulds.

We found this freestone and sea sand on the top of Montmartre, at Romainville, at Saint Prix near Montmorency, Longjumeau, &c. In these last we also remark fossil balani.

We cannot help reflecting, when looking at these freestones, filled with the same shells as those of Grignon, on the singular circumstances that must have presided over the formation of the strata we have examined. On beginning with the strata after the chalk, we may represent to ourselves in the first place a sea which has deposited an immense mass of chalk and mollusci of a particular species. This precipitation of chalk and the shells which accompany it, suddenly ceases. Strata of a totally different nature succeed it, and nothing else is then deposited but clay and sand without any organized body. Another sea returns: this last contains a prodigious quantity of testaceous mollusci, all of them different from those of the chalk. It forms at bottom thick beds, in a great measure composed of the testaceous envelopes of these mollusci; but this production of shells gradually diminishes, and also suddenly ceases. The soil seems then to have been covered with fresh water; alternate strata of gypsum and of marle are formed, which surround both the bones of the animals which these waters nourished, and the bones of those which lived on their shores.

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The sea seems to have returned a third time, producing some species of bivalve and turbinated shells; but very soon after the same sea gives birth to oysters only. Lastly, the productions of the lowermost second sea reappear, and we find on the top of Montmartre the same shells which were found at Grignon, and in the bottom of the quarries at Gentilly and Meudon.

Art. VI. *Formation of siliceous Limestone.*

The formation of which we have spoken is parallel, as it were, to that of the siliceous limestone. It is neither situate above nor below, but on one side of it, and seems to retain its place in the immense extent of soil which it covers to the west and south-west of Paris.

This soil is placed immediately above some plastic clays. It is formed of distinct beds of limestone, sometimes soft and white, and sometimes gray and compact, and very fine grained, penetrated with silex which seems to have filtered in every direction. As it is often porous, this silex, by filtering into the cavities, has fringed their sides with tufted stalactites (*mamelonées*) variously coloured, or with very short crystals of quartz, almost without any prism, but clear and limpid. This disposition is very remarkable at Champigny. The compact limestone thus penetrated with silex yields, on being burnt, lime of a very good quality.

But the distinguishing character of this singular formation, which no person had remarked before we did, although it covers a considerable extent of soil, is this, namely, that it contains neither *marine* nor *fluviatile* fossils; at least we have not been able to discover any in the great number of places where we have examined it with the most scrupulous attention.

It is in this soil that we find the stones known by the name of burrstones (*meulieres*) : these stones, the origin, formation, and situation of which were unknown to most mineralógists, seem to be the siliceous impression of the siliceous limestone. The silex, stripped of its calcareous parts by an unknown cause, must have left, and does in fact leave, porous but hard masses, the cavities of which still contain argillaceous marle, which present no trace of stratification. We have made true artificial burrstones by throwing this siliceous limestone into nitric acid. We shall explain in our full Memoir the various districts of soil which are formed of this limestone. We shall finish its general

history by saying, that it is frequently exposed on the surface of the earth, but frequently also it is covered with argillaceous marles, with freestone without shells, and finally with alluvial soil. Such is the nature of the soil of the forests of Fontainebleau.

Art. VII. *Formation of Freestone without Shells.*

The freestone without shells, in whatever place it is found, is always either the last or the penultimate formation. It constantly covers the rest, and is never covered, except by the formation of alluvial soil. Its beds are frequently very thick, and mixed with beds of sand of the same nature. The sand which supports the upper beds, has been sometimes washed out by the water; the beds are then broken, and have rolled over the flanks of the hillocks which they formed: of this kind are the freestones of the forest of Fontainebleau, of Palaiseau, &c.

Not only do this freestone and sand contain no fossils, but they are frequently very pure, and furnish sands much esteemed in the arts, and which are gathered at Etampes, Fontainebleau, Aumont, &c.

They are sometimes, however, either altered by a mixture of argil, or coloured by oxides of iron, or impregnated with carbonated lime which has penetrated them by infiltration when they are covered by the calcareous soil of fresh water: this is still the case with the freestone of several parts of the forest of Fontainebleau.

Art. VIII. *Formation of the Fresh-water Soil.*

This formation constantly covers all the foregoing. The rock which has resulted from it resembles, in point of structure and other external properties, siliceous limestone, *i. e.* it is sometimes compact, sometimes white and soft, but almost always penetrated with siliceous infiltration. The same silex, sometimes opaque and yellowish, sometimes brown and translucid like pyromatic silex, in some places completely fills the place of the limestone: finally, this formation gives, like the sixth, burrstones, the origin of which has one and the same cause.

What exclusively therefore characterizes this formation is, on the one hand the presence of shells, which are evidently of fresh-water origin, and similar in every respect to those which we find in our marshes. These shells are lymneæ and three kinds of planorbes. We also find in this formation

tion small round hollow bodies (*canelés*) which M. de Lamarck has called *gyrogonites*. We are not acquainted with any analogous existing species: but their position informs us, that the organized body of which they formed part lived in fresh water.

The second character of this formation is the facility which the limestone composing it has of being dissolved in water, however hard it may seem at the moment of its being taken out of the quarry. Hence it is used as marle, fertilizing the soil at Trappe near Versailles, and other places.

We refer to this formation, but rather with hesitation, the sands of the eminences which contain wood and parts of vegetables changed into silex. We were led to make this junction, by observing the siliceous wood and vegetables, which we find towards the top of the hillocks of Longjumeau. The same sand which contains these vegetables, also contains silex filled with coarse lymneæ and planorbes.

The fresh-water soil, although always superficial, is not found in every situation, but rather towards the summits of the eminences, and on the great platforms as well as in the bottoms of the valleys; if it exists in the latter situations, it has been covered by the soil which constitutes the ninth and last formation. Besides, it is extremely common throughout the whole of the environs of Paris, and probably at distances much further off than we have visited. It seems to us astonishing, that so few naturalists have paid attention to the subject; we know no other person than M. Coupé who has mentioned it.

The presence of this soil pre-supposes, in the fresh water which then existed, properties which we no longer find in those now in existence. The waters in our marshes and lakes deposit nothing but friable slime. We have not remarked in any of them the property possessed by the fresh waters of the old world to form thick depôts of yellowish and hard limestone, of white marles and silex, frequently very homogeneous, enveloping all the ruins of the organized bodies which lived in these waters, and even bringing them to the siliceous and calcareous nature of their envelopes.

Article IX. *Formation of Alluvium* (*Atterissement*).

Not knowing how to designate this formation, we have given it the name of *alluvium*, which indicates a mixture of matter deposited by fresh water. In fact, the slime of al-

luviation

luviation is composed of sand of all colours, marle, or even of the mixture of these three substances, impregnated with carbon, which gives it a brown and even black appearance. It contains rounded flints; but what characterizes it more particularly is, the remains of the huge organized bodies with which it abounds. It is in this formation that we find large trunks of trees, bones of elephants, of oxen, antelopes, and other large mammiferæ.

It is also to this formation that we may ascribe the accumulation of flints at the bottom of valleys; and probably also those of some plains, such as the Bois de Boulogne, the plain of Nanterre at Chateu, and certain parts of the forest of St. Germain.

This alluvium is not only found in the bottom of our present valleys, but it has covered valleys or excavations which have been since filled up. We may observe this arrangement in the deep cutting made near Seran, for the canal of Ourque. This cutting shows an ancient cavity, filled with the substances which compose the alluvium, and it is in this kind of marshy bottom that we have found bones of elephants and large trunks of trees.

It is to the existence of these ruins of organized bodies which are not yet entirely decomposed, that we ought to ascribe the dangerous and frequently pestilential emanations which are extricated from these soils, when they are stirred up for the first time since the period of their formation; for it is the same with this formation, which appears to be so modern, as with all those others which we have examined. Although very modern in comparison with the other soils, it is still anterior to any historic æra; and we may say, that the alluvium of the old does not in any respect resemble that of the present world, since the wood and animals found in them are entirely different, not only from the animals of the countries where they are found deposited, but also from all those hitherto known.

VII. *Ac-*

VII. *Account of an extensive Organic Lesion of the Brain, thoracic and abdominal Viscera, unaccompanied by the Symptoms usually observed in similar Affections. By* JOHN TAUNTON, *Esq. Lecturer on Anatomy, Surgeon to the City and Finsbury Dispensaries, and to the City Truss Society, &c.*

To Mr. Tilloch.

SIR, IN the course of my various communications to your Journal, I have more than once had occasion to describe cases of injuries to vital parts, which are generally supposed to produce a suspension if not a total deprivation of the reasoning faculties *.

In detailing the remarkable appearances I am about to lay before the public, it is not my wish to be regarded as espousing a doctrine contrary to the generally received opinions respecting the connexion that subsists between the organic structure and intellectual faculties of mankind:—I am anxious, however, that the anomalies which daily fall under the observation of medical practitioners may excite them to further inquiries. It has been my lot to witness cases in which the bones of the cranium have been so demolished, that large portions of the brain escaped from the wound; and on other occasions besides the one I am about to detail, I have found tumours of an inch and upwards in diameter, formed on the surface and substance of that viscus, and yet no symptoms of mental derangement, stupor, or even loss of sight, hearing, &c., supervened. On the other hand, I am free to admit, that I have seen lesions of this description, nay even the most trifling depressions of the skull, producing all the disagreeable consequences which we are taught to expect from injuries of the head. May there not be some chasm in the theory of physiologists on this point, which is reserved to future observers to fill up?

In November last I was called to a M. De la Roche, a foreigner, who described his age as 67. He detailed the circumstances of his case, with a firmness and precision, which perhaps no patient, labouring under such a complication of disorders, ever before evinced. On examination I found a fistulous sinus in the glutii muscles, which he informed me had existed for many years: he had long complained of great pain in the left cavity of the thorax,

* See Philosophical Magazine, vol. xxix. p. 169, and vol. xxx. p. 363.

often

often placing his hand on the affected part:—in other respects his health had been previously good. He was regarded as a most active man: he had a general knowledge of literature and science; spoke several languages; was distinguished for the facility with which he could converse upon most subjects, and reasoned so closely that his intellectual powers were generally regarded as of a superior kind. He had been repeatedly employed by the late Mr. Pitt on missions to various parts of the continent, and had invented several improvements in mechanics, which had gained him considerable notoriety.

About two months previous to my seeing him he had been seized with what was pronounced by a medical gentleman who visited him, to be peritoneal inflammation. This gentleman having discontinued his attendance, I was sent for by the patient; not so much with a view to administer any professional aid, as to express my consent to "anatomize his body" after his dissolution, which he never ceased to contemplate as rapidly approaching, with a firmness of mind peculiar to himself. This singular, and I believe almost unprecedented request, was even committed to writing by the patient, and delivered to a particular friend.

On further inquiry I learned that he felt considerable pain also in the abdomen, and on applying my hand externally I felt a tumour in the scrobiculus cordis, extending to about midway between that part and the umbilicus; but this tumour was not sensible to the mere casual touch. His pulse was good, and did not indicate inflammation; but he appeared to be gradually sinking; he took but little nourishment, and did not rest well.

The medicines administered were opiates with aperients, which afforded some temporary relief. A few days previous to his death he had several convulsive fits: he lost the use of his left side before death; but the circulation and heat in that part of his body were uninterrupted; the lower extremities at this time also became anasarcous. He continued perfectly sensible to the moment of his dissolution.

Examination of the Body.

On sawing off the upper part of the cranium, the bones appeared perfectly diaphanous;—the membranes and surface of the brain were in their natural state. The general substance of the brain was firm.

On separating the hemispheres, there appeared attached to the right hemisphere a dark-coloured tumour of at least

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an inch in diameter; it was of a granulated, fibrous and grumous consistence. Within the substance of the same hemisphere there appeared several other tumours, which, on being cut into, exhibited nearly the same appearances. The general substance of the brain was firm and sound: the olfactory nerves were uncommonly firm, not having the usual fibrous texture, but rather resembling pieces of narrow tape, of a dead white colour; the other nerves were equally firm.

In the thorax the left lung was entirely obliterated, apparently resulting from long continued inflammation. The right lung and the heart were in their natural state. The coronary arteries were ossified to a considerable extent*.

In the abdomen there was about three quarts of a dark-coloured serous fluid; the viscera were of a very dark colour. The structure of the alimentary canal was not injured: the kidneys, bladder, and spleen, were in the natural state: the liver was completely scirrhous: the pancreas was also scirrhous, which must have formed the tumour felt while the patient was alive. The gall bladder was much distended with bile.

There were no symptoms of a scirrhous pancreas during life.

With respect to the diseased appearances of the trunk in the above case, perhaps little doubt can arise; but it may not be improper, before I conclude, to call the attention of my fellow practitioners to the peculiar state of the brain.

The tumours in this organ were exhibited in various stages of progression to the size of the largest, and that which was first described. The whole of them must have existed for many years, and it will perhaps be difficult to explain—Why these tumours did not produce symptoms of compression, considering the space they occupied? Or are we to suppose that absorption of the surrounding parts took place in unison with the growth of the tumour?

I am, &c.,

JOHN TAUNTON†.

* Although this ossification was extensive, no symptoms of angina pectoris could be traced as having ever existed. Several preparations in my possession, when considered with reference to the histories of the patients, furnish results perfectly analogous.

† The preparations of the various diseased parts in the above case are preserved in my Museum, where they may be seen.

VIII. *On*

VIII. *On Dr.* Pearson's *Proposal for an Institution for obtaining an equal Temperature in Houses.*

To Mr. Tilloch.

Sir, I was much gratified by Dr. Pearson's letter in your last number, announcing the probable erection of a large building, capable of having its atmosphere kept at an equable temperature, for the use of consumptive patients. Though no medical man myself, I have more than once had my attention directed to this subject; and I have long been of opinion, that such a building as Dr. Pearson alludes to, would be, in most cases, a palliative, if not remedy, superior to the removals to a milder foreign climate, which are so frequently painfully undertaken. That a combination of the architectural and philosophical sciences existing in England is adequate to the erection of a building, in which the air could be constantly maintained of a temperature as mild as that of Montpellier or Madeira, and of which the superior equability would more than compensate for any slight deficiency in freshness and purity, there can be no reasonable doubt; and how infinitely more, if this could be effected, a quiet residence at home, surrounded by attentive friends, must contribute to the care and restoration of a sufferer by a consumptive attack, than a harassing voyage, succeeded by an uncomfortable sojourn amongst strangers, need not be pointed out.

But, though I entertain sanguine hopes that, under the auspices of Dr. Pearson and the eminent architect he refers to, the consumptive rich may be shortly accommodated amongst us with a splendid erection of another *Albany*, of which while the air is as mild and balsamic as that of southern France, the extent and arrangements will admit of the indulgence of fashionable habits, and combine within itself the luxurious enjoyments and comforts of home, with the healthful influence of foreign climes; it would greatly detract from my satisfaction, if I conceived that these blessings were to be confined to the rich, and that the construction of a splendid and expensive edifice was a *sine quâ non* to their attainment. I am persuaded, however, both from Dr. Pearson's hints and my own observations, that at least the remedy of a mild and equable temperature may be compassed by persons in the middle station of life, in their own houses, and at an expense not exceeding what they would of necessity expend in applications far less efficacious. The first essential seems to be merely a suite of two

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air-tight apartments and an anti-room opening into each other, and having no other communication with the rest of the house than through the anti-room. Of these the first would serve for a sitting-, the innermost as a lodging-room. It is quite obvious, that all attempts at maintaining an equable temperature must be nugatory, if the patient have to pass from his sitting- to his lodging-room through a cold windy passage or staircase; and if the atmosphere of both be not kept at the same degree of warmth. There are few houses of modern construction that could not supply two rooms on the same floor opening into each other: and as the anti-room, which is essential for the purpose of preventing a cold draught of air on opening the door of the outer apartment, and for renewing the air of that apartment gradually, may be very small, there are few houses which would not admit of its erection on the stair-head of the first floor. The next essential is to make the rooms air-tight. This may be readily accomplished by double windows, the outer closely caulked, by walls accurately plastered and papered, and doors tightly fitting and listed if necessary. An equable temperature is the third essential. In effecting this, fires in an open stove, seem clearly inadmissible, if from no other cause than this, that in an air-tight apartment it is almost impossible to prevent their smoking; besides which, it is equally difficult to regulate their heat properly. *Stoves* of all kinds are open to the last objection, and frequently cause an unpleasant and unwholesome smell, even when the smoke does not escape. *Steam* would doubtless be the most effectual and elegant, as well as simple and perhaps cheap mode of heating the apartments. All that is wanted is a plain and intelligible description of the mode of its application, which could be practised by any ordinary workman. Here I confess my ignorance: indeed my chief reason for now addressing you is to request of Dr. Pearson, or some other of your intelligent correspondents, the requisite information. We were told long ago, that some eminent cabinet-maker was able to warm a large suite of rooms, even the garrets of his warehouse, from the steam of a single copper; and I understand steam is applied also in warming the Royal Institution: so that the practicability of applying it in this way is undoubted. What is wanted is the manner of its application, on a small scale, in private houses. I should conceive that a boiler, which might be fixed on one side of the kitchen-fire, would heat water sufficient to warm the air of two moderate-sized rooms. From this

6 boiler

boiler a tin pipe might be conducted, being wrapped with wool, or some non-conducting substance, until its entrance into the apartment to be warmed, and then uncovered to give out its heat. If sufficiently long or wide, and made always to incline towards the boiler, I should conceive that the greater part of the steam would condense and run back into the boiler; but of course there ought to be some outlet or valve to prevent the pipe's bursting. The chief data wanting are the size of the boiler and quantity of water necessary to heat a given space; the proper diameter of the pipe, the length to which it ought to extend in the room to be warmed, and its situation, whether near the floor or the cieling, &c. &c. It is clear, that as little of the steam ought to escape uncondensed as possible; and for this purpose, the tube in the room should be either very long or very wide, but which I know not. If the apparatus could be so contrived that the condensed water would return to the boiler, it would be a material point; for the great difficulty in adopting such plans is the impossibility of getting servants to attend to any directions which require frequent and precise observance. It is on this account that the steam-boiler should be immoveably fixed, and constantly heated without any particular care.

A steam apparatus, something on the plan above described, would, I conceive, be far more effectual, as well as less troublesome, than the pots of hot water temporarily used by Dr. Pearson: and a simple and cheap mode of applying it is highly desirable, not merely as a mode of heating the apartments of consumptive patients, but for general adoption in many other cases; in particular, for heating rooms where collections of plants, &c. are to be kept dry, but free from the dust and dirt which an open fire never fails to make. In concluding this part of the subject, I wish to inquire whether the Pennsylvanian stove, invented by Dr. Franklin, and which, from his discoveries, seems to combine the warmth and cleanliness of a stove with a sight of the fire, so essential to an Englishman's comfort, has ever been adopted in this country, or had a fair trial given to it?

Though there can be little doubt that a suite of rooms warmed in the manner indicated above would be a very good succedaneum during an English winter for an expensive voyage to Madeira or Lisbon, and though they who are impressed with a proper sense of the horrors of consumption would deem the consequent confinement as a trifling price for the advantage derived; it is not to be de-

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nied that it would be found very difficult to induce consumptively-inclined patients to forgo the pleasure of out-door exercise, and submit to such a confining regimen. But it has long struck me, that means might be devised to enable this numerous class, who purchase every sunny walk in winter with the risk of their existence, to enjoy their present liberty with far less danger. It is well known, that the great source of harm to persons with tender lungs is the sudden and great changes of temperature, especially from *cold* to *heat*, which no one who stirs out in an English winter can well avoid. The succession of a cold frosty air to the hot temperature induced by a large fire and tea; then perhaps a walk on the sunny side of a street, exchanged for the piercing cold of the shaded and exposed side; and the whole ended by entering into a hot room, and rushing to a large fire, where the irritability, or whatever we call it, of the body is extraordinarily condensed:—such is the succession which thousands are every winter repeatedly exposing themselves to: And can we wonder at the result to those who are of a consumptive habit? But how to avoid these changes and yet stir from home is the question. In one way this may be certainly effected:—by the application of a handkerchief to the mouth and nose, so as to prevent the air from ever passing into the lungs when in its coldest state, much of the danger of short excursions in the open air may be prevented. Of this fact I have had ocular demonstration in the case of a friend, who, before his adoption of this plan, had constant and severe colds through every winter, but, since he pursued it, has been nearly if not altogether free from them. And this gentleman is now able comfortably to accept winter invitations, which formerly he was under the necessity of declining, or of looking forward to with horror.

But would it not be practicable to invent a substitute for the pocket-handkerchief, which it is tiresome to hold, and too closely confines the breath? Might not a kind of mask consisting of a frame, which should closely apply to the lower part of the face, covered with three or four thicknesses of gauze, be very advantageously employed for the same end? Such a veil would, I conceive, constantly keep the lungs in an atmosphere never much below sixty, while the interstices of the gauze would readily admit of the requisite quantity of air, which, thus gradually mixed with the interior warmer mass, could never, even in the coldest weather, cool the membrane of the lungs so as to make it dangerous to come into a warm room, or approach a fire.

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The only objection that I can see, is the ridiculous figure that persons furnished with such masks would at first be thought to cut. But this would soon have an end. The adoption of such veils by a few of the great would be sufficient to induce even the robust to wear them; and in reality there would be much less to laugh at than there was at the first use of umbrellas, inasmuch as a man's lungs are somewhat better worth protecting than his coat.

I shall be glad if these hasty and indigested hints prove of any value in furthering the laudable views of Dr. Pearson.

I am, sir, your most humble servant,

January 15, 1810. CENEPS.

IX. *Notices respecting New Books.*

"*The Rudiments of Chemistry; illustrated by Experiments, and eight Copper-plate Engravings of Chemical Apparatus.*" *By* SAMUEL PARKES. Price 5*s*. in Boards.

THERE is much to commend in this little volume, which contains the principal chemical facts, detailed with precision and perspicuity, and illustrated with apposite experiments: but after the luminous discoveries of Mr. Davy,—discoveries which have entirely changed the relation and dependence of the various facts which constitute chemical science,—we cannot but express our surprise that Mr. Parkes should have adopted such an arrangement as the following:—Introduction; Atmospheric Air; Caloric; Water; Earths; Alkalies; Acids; Salts; Simple Combustibles; Metals; Oxides; Combustion; Chemical Affinity. In typography and mechanical structure the present work is an exact picture of Blair's *Grammar of Chemistry*, nor could a better model have been followed for an elementary work of this kind; but justice demands that we should add, it is far more correct in its detail of facts.

"*An Essay on the Effects of Carbonate, and other Preparations of Iron, upon Cancer: with an Inquiry into the Nature of that and other Diseases to which it bears a Relation.*" *By* RICHARD CARMICHAEL, *Surgeon. Second Edition, considerably enlarged and improved.* 8*vo. Dublin printed, and sold by* MURRAY, *London.*

Though subjects of medicine may find their way to the public by publications confined to that science, yet, as a branch of natural philosophy, we have always been ready to receive a selection. The disease to which this work refers

fers is too well known in its fatality to require any introductory remarks, but the remedies have for the most part been concealed from the public. Hence, solitary successful cases only having been published with care, the faculty have been unable to judge of the comparative value of a remedy they could know but imperfectly. Nothing therefore could be more desirable than a performance like Mr. Carmichael's, nor could any thing be ushered into the world with more modesty or propriety. "When I first published," says the author, "my Essay on the Effects of Carbonate of Iron upon Cancer, there was nothing I so much dreaded as the too sanguine expectation of the public, and that a remedy that succeeded in one instance would be required in every other to overcome this disease in all its stages, or be rejected as useless because it could not perform impossibilities. My own hopes were but moderate, and I was careful that they should not wander far beyond the certainty of my experience; but my experience was so circumscribed, that I could merely guess at the virtues of the medicine rather than appreciate its value. This is a misfortune I have not now to complain of;—many and various are the cases a short interval has brought within my care or observation—alike in their symptoms, however different their circumstances—and variable the event of success or disappointment. But if experience has taught me, that in particular instances the medicine may prove inefficacious, and *must*, where the ravages of the malady are great and extensive, yet I had almost universally the satisfaction of discovering its efficacy, wherever the cancerous mass was not very much enlarged: and even when this was the case, instances were not wanting of a perfect recovery, and seldom indeed did it happen that the disease was unalleviated by the medicine."

It is no small compliment to the author, that his proposed remedy has been pretty generally adopted by many of the most eminent of the London faculty, and that this adoption is becoming daily more general.

The plan pursued in the arrangement of the work is as follows:

1st. A detail of the most remarkable cases within the author's knowledge.—These are subdivided into such as were cured by iron—such as were alleviated, and such as were neither cured nor alleviated.

2d. The opinions of the ancients and moderns concerning cancer.

3d. Consideration of the nature of cancer.

4th. Treatment of cancer.

These are followed by some miscellaneous remarks on the predisposition to cancer, and its connexion with other diseases; and an attempt to answer the queries of the society formed in London for the cure of cancer.

The cases of cancer cured by preparations of iron amount to 30, all well authenticated; and many of them communicated to the author from various respectable sources.

The number alleviated is eleven, and those in which the remedy produced no good effects are seven.

We shall pass over the account of the ancient opinions concerning cancer, and even of the moderns. Of the latter our author remarks:

"Theories founded on such uncertain arguments scarcely deserve attention, only that they prove how frequently the symptoms of this disorder have given the notion of its production by the action of living animals. They seem to have nearly dropped into oblivion, till revived by Dr. Adams, who supports with much ingenuity and appearance of truth, opinions peculiar to himself on this subject. As the foundation of this theory, he premises, that hydatids possess the simplest form in which animal life can well be supposed to exist; and as the experiments of Doctor Hunter only prove in them a contractile force, which is allowed to be sufficient evidence of their life; so if a similar property can be proved in the contents of a cancerous tumour, *their* separate vitality is equally deducible.

"In cancerous breasts, he remarks, there is always found a quantity of *yellow greenish fat*, which is contained in cysts, these together he denominates Carcinomatous Hydatids; and to prove their contractile power, he directs the following experiment to be made. 'Immediately after the operation, take the amputated part, and cut it in a transverse, or indeed in any direction, and wherever you discover this fatty appearance, you will see the surface at first smooth under your knife. In an instant after you will find a papillary appearance all over this yellow green surface. Each of these papillæ you will find the contents of a capsule, the contraction of which has produced this conical figure.'

"The other observations in support of his theory chiefly tend to prove, that those parts usually affected with cancer are endued with but little powers of life; and which therefore we would, *à priori*, suppose to be best adapted for the nidus and support of beings possessing a separate existence. Among those are the organs subservient to the preservation of

of the species and not necessary to the existence of the individual, as the breasts and uterus in women and the testicles in men, which are most suceptible of the disease at that period of life when they become useless, and consequently possess but a small portion of vitality; however, that the cancerous predisposition may be anticipated by injuries, which render them entirely or in part incapable of performing their usual functions.

"As to the scirrhous structure which forms the most considerable portion of cancer, and possesses somewhat the appearance of softened cartilage, he argues, that 'if carcinomata pass through the same stages as Dr. J. Hunter has remarked of the common or lymphatic hydatid, is it not probable that on the death of any of them suppuration will follow, and that this suppuration may expose the living hydatids in such a manner that many of them may die from not being surrounded by living animal matter? To prevent this, he conceives a fungus is formed, which incloses individuals or clusters of them in separate compartments, so that the death of one set produces no effect on the rest.' However, the Doctor does not inform us whether this fungus is produced by the hydatids for their own preservation, or by the surrounding parts for the purpose of preventing the departure of those troublesome visitors; if the latter is the case, it at least proves, that we do not always profit by the assistance of the vis medicatrix naturæ.

"But when the mind rests a length of time on any favourite theory, it is too apt to seize only the evidence that may confirm, and to neglect altogether that which may subvert the opinions it has formed: thus the ingenious author, who has the merit of opening a new field of inquiry, overlooks some obvious circumstances that are strongly adverse to his theory, which appears to me to have its foundation in the following circumstances; viz. the colour, the quantity, and the consistence of the yellow greenish fat above taken notice of, together with its accumulation in capsules, and the papillary appearance expressive of motion observable on dividing it by a transverse section.

"But these appearances seem to be merely the effects of the deranged actions of the animal œconomy; except the one evincing a contractile power in what he terms capsules, but which, notwithstanding repeated investigation, I never could perceive. The colour of the fat in cancerous breasts, that Doctor Adams seems to lay so much stress on, may be produced by animal hepatic air, which Doctor Crawford has

has proved to be 'capable of imparting to the fat of animals recently killed a green colour,' and that this very air united with ammonia escapes in great abundance from cancerous as well as other malignant ulcers *."

"I have thus," says our author, "extensively considered the opinions of Dr. Adams, which, notwithstanding the objections that occur, appear to explain the phænomena of the disease more satisfactorily than any that have yet been offered; and I confess I cannot but agree with him in the fundamental part of his theory, *the independent life of cancer*; but my sentiments are somewhat different concerning the part in which that life is resident."

Mr. Carmichael then proceeds to offer his opinion of cancer as a parasytic animal (a term, we believe, first used by Doctor Adams). This leads to a very interesting history of parasytic plants and animals, which he traces through all the writings of Darwin, Willdenow, Hunter, and several others.

In the succeeding chapter the author enters more particularly into the evidence of the vitality of cancer. The first of these is, that the cancerous substance has no communicating vessels with the parts in which it grows, and the insensibility of the person in whom it finds a nidus to any injury confined to the cancerous mass. 2d. That carcinoma arises in parts naturally endued with little life, or which, from their nature, are more inclined to run into decomposition, particularly the organs in each sex subservient to generation, after the period is passed in which they can be used for such purposes. 3d. From the fair presumption that when suppuration takes place, it is not of the cancerous substance itself, but of the neighbouring parts, which are stimulated to suppuration by the previous death of one part of the carcinomatous mass, according to a law first discovered by Mr. Hunter, that a living animal confined within the substance of another animal does not stimulate to suppuration; but that the same animal when dead stimulates like any other extraneous substance. 4th. "The origin of carcinoma first commencing in a point—the formation of cysts in its texture, containing a fluid—those cysts evincing a contractile power, by a forcible expulsion of their contents on being punctured, are all circumstances which strongly impress the idea that carcinoma is possessed of individual life." In this division the author enters much

* Experiments and Observations on the Matter of Cancer, &c., by A. Crawford, M.D.

into

into the general relation between hydatids and cancer. 5th. The locality of the disease. 6th. The peculiar kind of pain, which is sometimes compared by the patient to the gnawing of an animal.

"Such are the facts," concludes Mr. C., "that lead me to adopt the unnatural hypothesis that cancer enjoys an independent animal existence in the body on which it preys; and when another theory offers itself that so precisely meets every circumstance, and so perspicuously disentangles every difficulty of this obscure and intricate disease, I shall without hesitation relinquish opinions that I confess are difficult to digest, and most difficult to him whose reading has been most extensive. Vain indeed would be any effort to render them palatable to minds versed in systems of every branch of philosophy, if they had not previously learned the vanity of all human knowledge, and the futility of attempting to embrace within systems the infinite variety of nature."

We cannot help remarking, that most if not all these arguments in favour of the vitality of cancer are urged by Dr. Adams. The second indeed—that carcinoma arises in parts naturally endued with little life, is somewhat differently stated by that author. In accounting for the breasts, ovaries, uterus, and testicles becoming the nidus for hydatids in general, as well as for what he calls carcinomatous hydatids, Dr. Adams assumes as a cause, that such parts are not necessary for the existence of the supporting animal, and that after a certain time they become useless for those offices for which only they are destined.—That by injury that uselessness may be anticipated, and that in some females who are barren, the parts *ab initio* being useless, the breast becomes the nidus for cancer at a very early period.—The last argument produced by Mr. Carmichael we think might be omitted. Gnawing pain is a very common term for rheumatism—darting is a much more usual description for cancer among the unhappy sufferers. We mean not by these remarks to detract from the candour or merits of the author. He seems to have omitted scarcely any other opportunity of introducing Dr. Adams's name, and always with respect: and as to his own opinions, it must be admitted that they stand on the fairest ground—the success of a generally adopted practice.

Mr. C. next enters on the "treatment of cancers." In this most important part, and which, notwithstanding the envy of some of our contemporaries, we think we may truly call his own, as far as any philosophical view of the

 subject

subject extends, we are ready to give every credit to the ingenious author. Our limits will not permit us to follow him through the whole, but we cannot help remarking, that every part shows equal industry and genius.—After remarking the various unsuccessful attempts hitherto made, he engages in an inquiry concerning the uses of iron in the system—The diseases arising from an excess of the oxide of iron—Those arising from a deficiency of the same From all these, many valuable inductions follow on cancer and some other equally deplorable diseases.

The work concludes with an attempt at answering the queries of the medical committee of the society for investigating the nature and cure of cancer, and the plan of a hospital in Dublin for the reception of cancerous patients. Of the first, we cannot help lamenting that we hear so little of a society which seemed at one time to promise so much. The author's answers of course respect principally passages referred to in his work. Of the second, we cannot help regretting that the inconvenience remarked by Dr. Adams, in his Treatise on the Cancerous Breast, should be so soon forgotten. Can there be a more dreary prospect for cancerous subjects, than to be constant witnesses of its frequent fatality, and the agonies of their fellow-sufferers? Would not the funds of such an institution be better employed in supporting the patients among their friends, and administering remedies, than in erecting expensive buildings? We cannot, by these few remarks, be suspected of a wish to detract from the merit of a work which does equal honour to the head and heart of the author.

Practical and descriptive Essays on the Economy of Fuel and Management of Heat. Essay First. By ROBERTSON BUCHANNAN, *Civil Engineer. 8vo. With 2 Engravings.*

The public having given a very favourable reception to a short *Essay on the Warming of Mills and other Buildings by Steam*, published by Mr. Buchannan in 1807, and of which we took notice in our xxixth vol. p. 272, instead of merely reprinting that Essay, he has been induced to extend his plan to a series of Essays under the above title, of which, the first, consisting of 280 pages, has just made its appearance. It is divided into three parts. PART I.—Effects of Heat—Means of Measuring it—Fuel, &c.—Section 1, Heat, Thermometers, Tables; 2, Expansion of Solids and Liquids, Tables; 3, Specific Heat of Gases, Liquids and Solids,

Solids, Tables; 4, Combustion, various Kinds of Fuel; 5, Motion of Heat on the conducting Power of Bodies, Refrigeration; 6, Ebullition, Steam, Tables of Expansion of Air, Water, and other Liquids; 7, Ignition. PART II. —Heating Mills and other Buildings by Steam:—Section 1, Proportionate Size of Boilers; 2, Proportion of Steam-Pipes for Heating a given Space; 3, Substance and Surface of the Pipes; 4. General Observations respecting Arrangement, &c.; 5, Of the Method of connecting the Pipes; 6, Description of the Boiler with its Apparatus; 7, Syphons; 8, Arrangements in actual Use in Mills, Dwellings, and Baths. PART III.—Drying and Heating by Steam applied to Manufactures, &c.

The volume before us exhibits in a condensed yet perspicuous manner the principal laws which regulate the phænomena of heat, including all the recent discoveries of modern philosophers, and the best practical application of them which we have yet met with to the various purposes coming within the plan of the author. To civil engineers, and others whose professional avocations embrace such objects as Mr. Buchannan has illustrated, we cannot recommend it in terms beyond its merits.

Mr. T. Woodfall, assistant secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce, has announced his intention to publish, by subscription, in 2 volumes, 8vo, the whole of the very valuable papers on Agriculture which have been brought before the Society. The approbation given by the Society to these documents, which embrace every subject connected with agriculture, and extensive details of valuable experiments and observations, cannot fail to recommend the present undertaking to the notice of the public.

Mr. Ayshford, member of the Royal College of Surgeons, and assistant surgeon in the Royal Artillery, has in the press An Epitome of Anatomy, comprised in a series of tables. The work will form a thin quarto volume; and as its object is to furnish a copious vocabulary for the student of anatomy, perspicuity and simplicity of arrangement have been chiefly aimed at by the author.

X. *Pro-*

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

JANUARY 11,—The president in the chair. This society having assembled after the holidays, a summary of Mr. Home's observations on dissecting the man who lately died in the hospital in consequence of the bite of a rattle-snake was read. Mr. Home related all the symptoms from the time the patient was bitten till his death, which corresponded with those observed by Dr. Russel at Aleppo, and were; torpor in the part, swelled arm, pain, fever, delirium; low rapid pulse, generally 100, mortification and death after 18 days' suffering. On opening the body, the blood in the pericardium had a frothy appearance, and the skin of the arm separated entirely from the muscles, as observed by Dr. R. Mr. H. related two or three other cases of the bites of snakes terminating in the same manner and with similar effects.

The evenings of the 18th and 25th were occupied in reading a very long and important paper on the comparative qualities and strength of British and foreign culinary salt, by Dr. Henry, of Manchester. After stating the popular prejudice in favour of the supposed superior strength of foreign salt, compared with British, Dr. H. proceeded to detail the general process of preparing salt in different parts of England and Scotland, compared with the sea or bay-salt from St. Ubes, which is vulgarly believed to preserve meat better than English bay-salt. To ascertain if there could be any reason for this belief, Dr. H. analysed specimens from all the mines and manufactories of the common salt of commerce, and found their results so analogous, as to render any great difference in their curative powers almost impossible. The results of his experiments he drew up in a tabular form, exhibiting the constituent principles of the salt of various places; in 1000 parts he found the quantity of muriate of soda to vary only from 935 to 988, the lowest and highest in any culinary salt. In the foreign salt he generally found two or three parts more of muriate of soda than in the British; but this advantage was more than counterbalanced by the extra quantity of sulphates of lime and magnesia in foreign salt: the latter substances also exist in British salt, but generally in much less proportions, particularly the sulphate of lime. Hence Dr. H. concluded that our native salt is in every respect equal to foreign salt for the preservation of provisions, and that

that the vulgar prejudice against it should be instantly removed by every rational and practicable means, as injurious to the commerce and prosperity of the kingdom.

In the process of manufacturing, he observed that in the North of Scotland, where the fire to the pans is allowed to become low by neglect on Sundays, a species of very strong salt has been produced in consequence, which has obtained the name of "Sunday salt," which he thinks even more powerful than any foreign salt. The cause of this superiority he attributes to the slowness of evaporation, which makes the grain of the salt larger. Large-grained salt is best for curing dry meat, as it dissolves more gradually, and always affording fresh supplies of saline moisture; small-grained, on the contrary, is best for making brine. The decrepitation of all salt Dr. H. found nearly alike; the water of crystallization being in very small quantities, not exceeding two or three per cent. in salt dried at the usual temperature of 212. The specific gravity is also very little different; that of St. Ubes was 19·68, while the British varied from 20·23 to 20·88.

In concluding, the author related some of the tedious and complex operations which he adopted in these researches, in order to ascertain the relative and constituent principles of the various kinds of common salt. Luna cornea, or muriate of silver, was one of his principal tests; but the experiments to detect the sulphates of magnesia and of lime were tedious and complex, especially in ascertaining the presence of an ammoniacal sulphate of magnesia. Among many other curious experiments by this able operator, he ascertained the *compatibility* of sulphate of soda and sulphate of magnesia in the same liquid, contrary to the chemical axiom laid down by Mr. Kirwan. It was not, however, till after two days digesting that a very small quantity of these salts was found to be partially united, and from this experiment the author does not seem disposed to question the truth or utility of Mr. Kirwan's position in regard to salts in their natural state.

SOCIETY OF ANTIQUARIES.

Some curious particulars respecting the former perquisites at the Board of Green Cloth, and the conduct of Sir Gilbert Talbot, keeper of the king's plate during the reign of William III., were read. The only useful facts which this paper established were, that in former as well as the present times, avarice, intrigue, violent passions, and love of places and perquisites prevailed.

Mr.

Mr. Douce exhibited to the society a French marriage token, never used as a coin, and consequently not noticed in any work on coins or medals. On the one side it bore the circular inscription "Pour Epouse," round fleurs de lys, with a D at the bottom; and on the other "Denirs de Foy," with united hands. These tokens were formerly given in betrothing brides. Mr. D. quoted several decrees and ceremonies relative to the performance of marriage, and among others a decree of the council of Toledo, prohibiting the queens of Spain to marry a second time.

The Right Hon. Sir J. Banks, Bart. President of the Royal Society, communicated a curious parchment roll, exhibiting the marks made on the beaks of swans and cygnets in all the rivers and lakes in Lincolnshire, accompanied with an account of the privileges of certain persons keeping swans in those waters, and the duties of the king's swanherd in guarding these fowls from depredation, and preventing any two persons from adopting the same figures or marks on the bills of their swans. The number of marks contained in the parchment roll amounted to 219, all of which were different, and confined to the small extent of the bill of the swan. The outlines were an oblong square, circular at one end, and containing dots, notches, arrows, or such like figures, to constitute the difference in the marks of each person's swans. Laws were enacted so late as the 12th of Elizabeth, for the preservation of the swans in Lincolnshire.

LINNÆAN SOCIETY.

January 16.—Dr. Maton, vice-president, in the chair. Read a description of some new species of plants from New Holland, by Edward Rudge, esq., F.L.S. Part of a paper by William Spence, esq., F.L.S., was also read, on a genus of insects named Choleva by Latreille, with a description of eighteen British species, which was prefaced by some remarks on the comparative merits of the different systems of entomology.

XI. *Intelligence and Miscellaneous Articles.*

FLINT GLASS.

THE French artists still continue their exertions to manufacture flint glass to rival if possible that of this country. In our 33d volume we gave a report of a committee appointed by the French Institute to examine some attempts of

of this kind made by a M. Doufourgerais. A similar report has been recently published by the Institute on a specimen of flint glass presented by Messrs. Kraines and Lançon. It is described as of great purity, and totally devoid of striæ: its specific gravity is to that of the English flint glass as 37 to 33: dispersive powers very great, being as 5 to 2 with common glass, while the proportion of common glass to flint glass is 2 to 3. Its refraction being described as very strong, the foci of the glasses made with this glass are one-fourth shorter than common glass. M. Delambre informs the Institute that he has made experiments with an achromatic glass of the above materials, and pronounces it to be far superior to a telescope of equal size made by Dollond.

HERNIA.

In a recent foreign journal the following new remedy for hernia has been proposed by a M. G. Tarenne:—

"The author of this proposal has made a number of experiments on snails, and on the singular properties of their slimy juice, which has for a long time been used with success in disorders of the breast. The viscidity of this juice, its astringent virtue, and its reproductive faculty, induced him to suppose, that when applied externally in certain infirmities it would easily penetrate the skin, and spread itself throughout the part affected. He presumed that this juice would in some way close hernial openings; and to assure himself of this property he undertook the care of several persons afflicted with hernia. He had the happiness to succeed in curing them radically in the space of three months.

"This discovery appearing to him too important to be kept secret, he did not hesitate to publish it. We are obliged here to abridge his manner of proceeding, and refer our readers to the work published by M. Tarenne, entitled 'Cochliopérie; Recueil d'Expériences très-curieuses sur les Hélices terrestres, ou Escargot,' &c. 1 vol. 8vo. Paris, 1808.

"The first thing necessary is to be assured of the nature of the hernia by consulting some experienced surgeon. If the ruptured part cannot be returned by any means, or if it is dangerous to confine it in the body, this specific must not be made use of, as it would in this case only augment the evil.

"A truss is then to be made, having the ball at the end concave instead of convex, as is usual, to receive a kind of cup

cup of an equal diameter with that of the orifice of the hernia. This cup may be of porcelain, earthenware, or glass, in order that the liquor which it is to contain may not penetrate it, nor lose any of its virtue, nor undergo any alteration. The edges of this cup are widened a little, that they may not incommode the patient when it is placed in the truss. It is to be filled with wool, which must be changed every other day.

"About two, three, or four hundred snails, according to the size, are then to be procured, and kept in a place where they may derive nutriment, because only two or three are used every day, or six or eight if they are small. They are more easily procured, and of a better quality, in spring, which is the most favourable season for this business.

"The patient every day before he rises, and after he is in bed, takes away the cup from the truss, and with a pin wounds the snail at intervals in different places. From each wound the snail gives out, through the opening in his shell, sometimes a blueish sometimes a gray-coloured water, which must be caught on the wool in the cup. If the snail only gives out a thick froth, it must be thrown aside and another taken instead.

"The cup being sufficiently filled with liquor is to be placed on the part affected, always very exactly in the same situation; it is then to be covered with a white linen cloth, and apply on it the ball of the truss. This truss, without being too tight, must be sufficiently so to prevent the fluid from escaping between the edge of the cup and the skin in any posture.

"During this treatment, which will last three or four months or more, the patient need not be kept to any particular regimen. The only precautions necessary, are to shave the part once every four days, and never to leave the hernia long uncovered, in order to avoid cold. To press more or less with the hand on the truss whenever the patient is going to cough or sneeze, or make any effort whatever. If the cup rubs off the skin on account of being badly made, or on account of the hair being suffered to grow too long, this treatment must be suspended until the skin is well again. In this case the patient will take away the truss altogether, if it can be done without the intestines escaping through the opening; this will depend on the position in which he is accustomed to lie in his bed. During the day he will wear the truss dry, by filling the concavity with wool and putting a bit of cloth on the hernia.

"By this kind of treatment a common hernia may be cured

cured in three or, at most, four months, unless something else ails the patient which tends to prevent the aperture from closing. The cure will be found complete when, by applying the finger to the place, we find it closed or almost closed.

"Although the aperture be closed, it will be proper for the patient to continue wearing his truss six weeks or two months longer, because it is indispensably requisite that the wound should be left to close before the muscles which encompass the place are permitted to enjoy their natural action. An unforeseen effort may be productive of more mischief than had occurred heretofore, if the opening be abandoned too early to itself whilst moistened with this antiherniacal liquor." S.

LECTURES.

St. Thomas's and Guy's Hospitals.

The Spring Courses of Lectures at these adjoining hospitals commence the beginning of February, viz.

At St. Thomas's.—Anatomy and the Operations of Surgery, by Mr. Cline and Mr. Cooper. Principles and Practice of Surgery, by Mr. Cooper.

At Guy's.—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt at the respective hospitals.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Cotton of Limehouse, manufacturer, for a new and improved method of regulating the texture of all kinds of cloth in the process of weaving.—Jan. 15, 1810.

To William Murdock of Soho Foundry, in the county of Stafford, engineer, for a process for boring and forming pipes, cylinders, columns, and circular disks out of solid blocks and slabs of stone of any kind or description.—January 15.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For January 1810.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec. 27	43°	39°	38°	29·82	0	Rain
28	35	38	38	30·05	10	Fair
29	43	47	43	29·53	0	Rain
30	43	46	44	·70	15	Cloudy
31	47	50	46	·90	12	Cloudy
Jan. 1	47	49	48	30·10	10	Cloudy
2	47	47	44	·15	0	Small rain
3	44	49	45	·14	15	Fair
4	45	47	44	·29	0	Small rain
5	46	45	42	·30	9	Cloudy
6	43	47	41	·31	10	Cloudy
7	40	43	40	·20	14	Cloudy
8	39	40	40	29·95	12	Cloudy
9	46	47	41	·80	4	Rain
10	40	44	40	·90	10	Fair
11	45	46	43	·85	9	Cloudy
12	43	44	40	·86	10	Cloudy
13	34	34	26	·91	12	Cloudy
14	27	29	24	·92	14	Fair
15	27	28	23	·85	10	Fair
16	20	26	19	·70	0	Snow
17	18	30	26	·96	7	Fair
18	28	34	25	30·20	0	Snow
19	25	29	24	·21	2	Cloudy
20	20	22	29	·05	0	Foggy
21	30	31	30	29·96	7	Fair
22	31	34	34	·90	0	Cloudy
23	34	38	35	30·08	0	Cloudy
24	35	36	35	·19	0	Cloudy
25	35	35	34	·23	4	Cloudy
26	32	35	33	·20	5	Cloudy

N. B. The Barometer's height is taken at one o'clock.

XII. *Description of a new Cupping Instrument. By* Robert Healy, *M.B., Dublin.*

No. 1, Clarendon Street,
Dublin.

To Mr. Tilloch.

Sir, I take the liberty of communicating to you a new method of performing the operation of cupping, without the assistance of the syringe, which I have tried, and find to succeed. If you think it worthy of holding a place in your very instructive Magazine, your inserting it will oblige your obedient servant, Robert Healy, M.B.

Description of the Instrument.

The instrument consists of two parts, namely, a hollow vessel A, (Pl. II.) which may contain about half-a-pint of water, made of very thin sheet-copper, or tin, with a stop-cock soldered into it. The end of the stopcock should extend a quarter or half an inch within the vessel A, for a reason that will be assigned. The other part of the instrument is the cupping glass B, made in the usual way, which is to be adapted to the stopcock, with a screw of a coarse thread.—The instrument is to be used in the following manner: the glass is to be unscrewed from A, a little air is to be drawn from the latter by the assistance of the mouth, the cock is then immediately to be turned to prevent the external air from rushing in. Ether or spirits is to be placed in a wine-glass. The mouth of the stopcock is then to be inverted into the liquid so deep, that by turning the cock a sufficient quantity may be drawn up: a drachm of either liquid will be found sufficient. The vessel A is next to be heated to convert the liquid into vapour: as soon as the vapour has filled the vessel, this must be refrigerated in a vessel of cold water, previously turning the cock to prevent the influx of air. A vacuum is produced in proportion to the size of the vessel, and in a very few minutes. The cupping glass is then to be screwed to the stopcock, and placed over the wound made by the lancet or leech. We may adjust the suction by turning the cock, and have a gradual or sudden flow of blood. If the vessel A be of large dimensions, and the patient complains of the suction, we must turn the cock, and either admit the blood to flow into the glass, or unscrew one turn of the ball from the glass, and admit the air to pass through the thread of the screw into the glass. The reason of extending the stop-

cock so far within the ball is to prevent the liquid in the ball from passing into the cupping glass when the stop-cock is opened.

November 14, 1809.

XIII. *On that Power of the Eye, by which it is adjusted to see Objects distinctly at different Distances.* *By* Ez. Walker, *Esq.*

[Continued from vol. xxix. p. 340.]

I. From observations made on the human eye at different ages, it appears to undergo a gradual change from infancy to old age. For when a child of three or four years old attentively views a very small object it is held close to the eye, not further off than two or three inches. The reason is this: in the early part of life the iris, being flexible, has the power of contracting the pupil to a smaller dimension than at any future perriod; consequently a child can see a small object at a less distance than a grown person: but by degrees this power of the iris decreases, as the rest of the body becomes less flexible; and the eye, if a good one, gradually becomes longer-sighted.

It is generally supposed, however, that the pupil of a child's eye is larger than that of a grown person's: but this is true only when the iris is in a state of relaxation; for it is well known, that the pupil always contracts in viewing a near object. To suppose that a child can see a small object at two or three inches distance with a large pupil is contrary to the laws of dioptrics, because the rays of light would then enter the eye in a diverging state, and consequently form an indistinct picture of the object upon the retina.

Dr. Jurin observes, that "children read much nearer than grown person's, for their eyes are smaller, and the least distance any person can see distinctly at, is proportional to the length of the eye*."

But to show that this rule is erroneous, let us suppose that the nearest distance of distinct vision of an eye is 18 inches; then this eye, according to the Doctor's rule, must be six times as large as the eye of a child that can see distinctly at the distance of three inches; which is contrary to experience. It also appears from the eyes of birds that the

* Essay on distinct and indistinct Vision, p. 147.

Doctor's

Doctor's rule is erroneous; for it is well known that large birds of prey can see objects at a distance, far beyond the limits of distinct vision of the human eye; and there are small birds that can see minute objects at a very great distance.

Now, as the eyes of birds are much smaller than those of men, it is evident that the utmost distance of distinct vision does not depend upon the length of the axis of the eye.

The nearest distance at which a grown person with a good eye can see distinctly, may in general be about six or seven inches; but this distance increases with age, until near objects appear indistinct.

This increase in the focal distance of the eye is generally supposed to be owing to the humours of the eye becoming too flat through age, so that the rays which fall upon the eye, from an object at a short distance, converge to a point behind the retina, and thus cause indistinct vision. But this theory is, I believe, unsupported by a single clear and decisive experiment.

That the humours of the eye may waste and the eye grow flatter, when every other part of the human frame is upon the decline, may be supposed with some appearance of reason; but that the humours of the eye of a child should decay and the eye grow flatter while every other part of its body is advancing towards perfection, is a theory which cannot be so readily admitted. For, as the eye undergoes a gradual change through life, this effect must be produced in every stage of it, by the same cause. Hence this theory requires further investigation.

II. Those who have written on the properties of the human eye inform us, that "if a good eye views an object at the least distance it can be seen distinctly, and then at twice that distance, and then at an infinite distance, there is about the same alteration made in the figure of the eye between the two last cases, as there is between the two first.

"For let B C D E (Pl. II.) be the axis of the eye infinitely produced; B C, B D, B E, the three distances of the object from the cornea A B; and C A, D A, E A, three rays falling upon any given point of the cornea; whereof E A is parallel to the axis.

"Now to produce distinct vision of the points C, D, E; it is plain that every one of the rays C A, D A, E A, must be successively refracted to the same point F, upon the retina, where it is cut by the eye's axis. At first let us sup-

 pose

pose the point F to be given, or the length of the axis B F to be immutable, and then the quantity of the refraction of each ray must be varied. And because the distance C D is supposed equal to C B, or C A, the angle C A D is equal to C D A, and consequently to D A E. Therefore, conceiving each ray to come back again from the fixed point F, to the points C, D, E, successively; the whole quantity of its refractions must first be lessened by the angle C A D, and then by the equal angle D A E; and so the changes of the figures of the refracting surfaces must be much the same when the object is removed from C to D, as when it is removed from D to E*."

Hence it may be demonstrated, that objects at various distances, as C, D, and E, may be seen distinctly without any *alteration in the humours* of the *eye*, or in its *outward form*.

1. It has been proved in a former paper †, that when a good eye views an object at the least distance it can be seen distinctly, the rays which enter the pupil are parallel, or such as differ very little from being so; whence it is evident, that the rays from the object C falling upon the eye at B are parallel.

2. Rays issuing from the object E, a planet or a star, at an infinite distance are parallel, consequently the angle D A E is infinitely small: and

3. As the angle C D A is equal to the angle D A E, the angle C D A is infinitely small; therefore the line D A coincides with the line D B, and the rays falling upon the eye from D must also be parallel; consequently the rays from the object D, and also those from E and C, are refracted to the same point F upon the retina: for parallel rays falling upon the eye near the axis of vision have the same focus, whether they come from objects that are near or remote. Whence it is evident that vision perfectly distinct is produced only by parallel rays; but the means by which the eye admits such rays only as are parallel, or nearly so, and rejects the rest, come next to be considered.

III.—If a small circular object be viewed with too large a pupil, which may be done by placing the object much within the limits of distinct vision, it will appear larger than perfect vision would represent it, by a penumbra of light, which is called the *circle of dissipation*.

Now if the pupil be contracted, by means of a perfora-

* Smith's Optics, Remarks, p. 2.

† Philosophical Magazine, vol. xxix. p. 342.

tion

tion made in a card of such magnitude as to prevent the lateral rays from entering the eye, the object will appear distinct. But if the aperture in the card be made as large as the pupil, the *circle of dissipation* will appear as large as before.

This circle of dissipation is formed by those rays which enter the eye remote from the axis of the crystalline lens. Thus, when the pupil is too large for distinct vision, the most refrangible of those side rays will cross one another in the vitreous humour, and, by falling upon the retina in a diverging state, will be dispersed over a larger space than the true image, and consequently form a penumbra round it; and the least refrangible rays of the same pencil will be dispersed over the interior parts of the circle; whence that indistinctness of vision which is experienced by people advanced in years.

No writer has paid more attention to the theory of distinct and indistinct vision than Dr. Jurin. This philosopher says, that "the radius of dissipation is, *cæteris paribus*, always proportional to the radius of the pupil. Consequently, when the pupil is narrow, the *radius of dissipation* and the *penumbra* arising from the dissipation will be smaller, that is, vision will be rendered either distinct, or at least less indistinct than it would otherwise be *."

IV.—Many philosophers have maintained that we have the power of viewing objects at different distances, by a conformation of the eye for this purpose, independent of a variation in the pupil; but they vary much in their opinions respecting the means by which this effect is produced.

Dr. Matthew Young says, that "the power of seeing distinctly at different distances does not depend on the crystalline.

"This is evident," he says, "from the experiments made on a person who had been couched for a cataract, and by the assistance of the same convex lens, applied to that eye, could see distinctly at different distances †."

But Dr. Porterfield is of opinion, that the change made in the eye must be in the crystalline; for a person who had been couched of a cataract was under the necessity of using glasses of different degrees of convexity, for seeing objects distinctly at different distances ‡.

These two alleged facts may both be true, but the con-

* Jurin's Essay on distinct and indistinct Vision, p. 145.
† Dr. M. Young's Analysis of Nat. Phil. p. 375.
‡ See Porterfield on the Eye, vol. i. p. 434, 435.

clusion drawn by Dr. Porterfield seems to be erroneous (although it has been used, not only by himself, but by other writers, as an unanswerable argument to prove that the eye is adjusted by means of the crystalline lens,); for if the iris of the person's eye mentioned by Dr. Porterfield received any injury from the operation of couching, the adjustment of that eye would be imperfect, and require glasses of different degrees of convexity for viewing objects at different distances; as it is well known, that persons who read with spectacles of very convex lenses require glasses of a less degree of convexity to view remote objects; still the less convex, the more remote the object.

But if the operation mentioned by Dr. Young was performed without doing any injury to the iris, that person would still be able to see objects distinctly at different distances, by the assistance of the same convex lens applied to that eye; because the iris is the only organ by which the eye is adjusted to distinct vision. It is a physical truth, which any one may convince himself of by trial, that no mental exertion can change the adjustment of the eye, when every part of the iris is covered. This may be tried by viewing objects through a hole in a card, made rather less than the pupil in its most contracted state.

V.—Sir Isaac Newton was of opinion, that the humours of the eye decay or shrink by old age, which causes the eye of the short-sighted to grow flatter till it comes to a due figure. For short-sighted men see remote objects best in old age*. And this opinion has been adopted by some of our best writers on the theory of vision. But let opinions give place to facts.

Mr. Adams says, "It is generally supposed, that the short-sighted become less so as they advance in years, as the natural shrinking and decay in the humours of the eye lessen its convexity, and thus adapt it better for viewing distant objects: but among the great number of short-sighted that I have accommodated with glasses, I have ever found the reverse of this theory to be true, and the eyes of the myopes never required glasses less concave, but generally more concave, as they grew older, to enable them to see at the same distance †."

Hence it is evident, that the humours of the eyes of the short-sighted undergo no change as they grow older, and that vision generally becomes less perfect. For, as the iris loses some part of its contracting power with age, the pu-

* See Newton's Optics, p. 13.

† Adams on Vision, p. 126.

pil

pil becomes larger, the *circle of dissipation* increases, and consequently remote objects appear less distinct than they did in the early part of life.

VI.—From this investigation it manifestly appears, that the eye is in reality no more than a machine of a fixed and determinate form, without any power to alter its outward dimensions, or to move any of its internal humours; and that the only adjustment necessary to form a distinct picture of an object upon the retina (whether the object be near or remote) is to prevent diverging rays from entering the eye, and to admit such only as are nearly parallel.

This office is performed by the iris, which contracts the pupil to exclude the side rays when we view near objects, and enlarges this aperture to give us a distinct view of such remote objects as are but faintly illuminated: it is also well known, that whenever the eye is exposed to a strong light, the pupil contracts, but it expands as the light decreases.

These contractions and dilatations of the pupil, according to the distances of objects and strength of light in which they are seen, are directed by that volition of the mind which presides over and regulates all the other motions of the eye.

Ez. Walker.

Lynn, January 3, 1810.

XIV. *On a native Arseniate of Lead. By the Reverend* William Gregor *Communicated by* Charles Hatchet, *Esq., F.R.S.**

I.

That the oxide of lead and the arsenic acid might be found in the state of natural combination, is a supposition highly probable, from the strong affinity which subsists between these two substances. But the existence of such a compound has not, as I conceive, hitherto been established by such proofs, as entitle it to be ranked amongst the decided cases of mineralogical science. I trust, therefore, that the observations, which I have the honour of submitting to the Society, on a new † ore of lead lately discovered

* From Philosophical Transactions for 1809, Part II.

† It is new at least to the miners in Cornwall; nor was there, previously to this discovery, any ore resembling it to be found in that splendid collection of minerals, which my valuable friend Philip Rashleigh, esq., has so liberally formed, and as liberally employed in the promotion of science.

 in

in the county of Cornwall, so justly celebrated as well for the variety as for the richness of its mineral productions, will not be deemed superfluous.

This mineral was raised in the mine called Huel-Unity, a very rich copper mine, in the parish of Gwennap. According to the information with which I have been favoured by Mr. William Davy, a very intelligent and experienced miner in that district, it was found in a lode south of Huel-Unity principal lode, at the depth of fifty fathoms below the surface, which lode underlay about two feet in the fathom south: at the depth above mentioned, this lode fell in or formed a junction with another small lode or vein to the south, and when the junction took place this lead ore was found. The veins of it are, in general, from six to ten inches wide, and they diverge on going west. Some particles of this lead ore have been found in the southern part, after the separation of the lodes; but the northern lode does not contain any until the junction takes place. This ore is intermixed with some native copper, very rich gray copper, and black copper ore, and some is mixed with quartz. The walls of both veins are killas.

II. *Description.*

This mineral is regularly crystallized. The form of its most perfect crystals is an hexahedral prism; they are of different sizes, from one-tenth of an inch in diameter to the size of a hair. The longest which I have seen do not exceed three-tenths of an inch in length: these terminate in a plane, at right angles, with the axis of the prism; but the crystals of a smaller size are frequently drawn out into a very taper acumination, which appears to be a six-sided pyramid. A number of smaller crystals are often closely packed together in bundles, which are bent in different directions, and terminate in a point. The larger crystals either stand alone, or adhere, on their lateral planes, to the gangue, or are confusedly matted together in a mass.

Some of them are hollow, as if an internal nucleus had been destroyed; and sometimes this internal nucleus overtops the external laminæ. The gangue is a white quartz, which frequently exhibits on its surface the appearance of a partial decomposition.

The red octahedral copper ore, and the copper into which that ore passes, are often intermingled with the crystals of this lead ore and imbedded in them.

The colour of these crystals consists of a variety of tints of yellow. Some are of a beautiful wine yellow resembling the

the Brazilian topaz: this, in the greater number of specimens, passes into a delicate Isabella-colour: whilst, in other cases, we have the honey-yellow mingled with brown hues of different intensities: so that we meet with crystals resembling dark brown sugar-candy, or common resin.

Some of the crystals are beautifully transparent, whilst others possess this quality in part only, at their extremities, or in inferior degrees throughout their whole lengths.

The external lustre, in some specimens, is vitreous; in others, resinous: but in some instances their surface is partially covered by tender and delicate filaments of a silky lustre. These filaments are sometimes found in a separate state loosely adhering to quartz; and they form a variety of this fossil.

The crystals vary as to hardness. The angular fragments of the most transparent are sufficiently hard to scratch glass.

This mineral is easily reduced to powder, which has the appearance of pounded resin; it contracts a yellower tint by long exposure to the air.

The specific gravity of the purest crystals, taken at the temperature of 50° Fahrenheit, was 6·41.

III.

A fragment of crystal, exposed to the flame of the blowpipe in a gold spoon, melted into a brownish-yellow mass, which on cooling did not assume any angular figure. It remained in a state of ignition apparently unaltered; but when a piece of it was exposed to the flame on charcoal, a rapid decomposition took place, arsenical vapours were extricated, and globules of a metal, possessing the common properties of lead, were left behind.

This mineral, in a state of fine powder, is soluble in nitric acid, even without the aid of heat. Care, however, must be taken, that it does not concrete into lumps. The vessel therefore which contains it must be frequently shaken, and the nitrate of lead produced must be, from time to time, dissolved in water, and poured off from the residuum. The process of solution is, however, accelerated by a digesting heat. Some silica remains, which, as the quantity of it is variable according to circumstances, appears not to be an essential ingredient of this fossil.

The nitric solution is colourless; its transparency is not disturbed by nitrate of barytes. Nitrate of silver renders it turbid, and a small quantity of white curdly matter is deposited. Sulphuric acid and the liquid sulphates produce

copious

copious precipitates of a white heavy matter. If the fluid be poured off from this subsided matter, and it be freed from the superfluous sulphuric acid, by the means of nitrate of barytes, it will yield, on the affusion of liquid nitrate of lead, an abundant white precipitate, which, urged by the flame of the blow-pipe on a support of charcoal, resolves itself into reduced lead and arsenical vapours.

These preliminary experiments led me to the probable conclusion, that this fossil chiefly consisted of oxide of lead, arsenic acid, and a small quantity of the muriatic acid.

IV. *Analysis.*

A.

1. Fifty grains, carefully selected from crystals of a pale Isabella-colour, were reduced to a fine powder, and exposed to a low red heat for about an hour. Their weight was diminished by 0·15 of a grain.

2. The yellowish powder was now transferred to a vessel of pure silver, and mixed with a lixivium containing fifty grains of potash, prepared by the means of alcohol; a quantity, which I had previously ascertained to be sufficient to effect a complete decomposition of this mineral. The ley was gradually evaporated to dryness in a sand-bath. The soluble part was extracted by distilled water, and poured off from a yellowish white matter, which was sufficiently edulcorated (*a*).

3. Liquid nitrate of ammonia was now dropped into the alkaline fluid, as long as it produced any cloudiness: the clear fluid was now decanted from a small quantity of white matter, which had subsided, and rendered acid by nitric acid; ammonia, added to excess, produced a slight turbidness. These precipitates, after sufficient edulcoration, were added to the yellowish white residuum (*a*).

4. The liquid was now rendered slightly acid by nitric acid, and a solution of nitrate* of lead in distilled water was dropped into it, as long as it separated any precipitate. The clear fluid was poured off, and evaporated nearly to dryness, and a small quantity of white matter, thus obtained, was added to the former precipitate, which dried,

* If the colourless liquid oxy-nitrate of lead be dropped into a dilute solution of arsenic acid, or of arseniate of potash acidulated by nitric acid, no immediate precipitation of an arseniate of lead is produced; but crystalline grains are, after a time, gradually deposited at the bottom of the vessel. But liquid nitrate of lead causes an immediate and abundant precipitate from these same dilute solutions. These two combinations therefore must be different.

and

and exposed to a low red heat, weighed, whilst still warm, 40·8, which, according to the proportion of 33 : 100, established by Mr. Chenevix, implies 13·46 of arsenic acid.

5. The superfluous lead was now separated from the fluid by sulphate of soda, and filtered off. Ammonia precipitated a minute portion of flaky matter; it weighed, after ignition, 0·2 of a grain; it consisted of silica and oxide of lead, and must be attributed to the nitrate of lead employed.

B.

1. The yellowish white residuum (*a*) (A, § 2.) was dissolved without effervescence in nitric acid, except a minute portion of silica, which, after ignition, = 0· 8. A white heavy matter was thrown down from this solution, by liquid sulphate of soda. The clear decanted fluid was evaporated to a small volume, and sulphate of soda produced a further separation of white matter; it was sulphate of lead, which, after exposure to a low red heat, and weighed, whilst warm, = 47·5, which, upon the supposition that one hundred parts of sulphate of lead contain 69·74 of lead + 3·48 of oxygen, is equivalent to 34·77 of oxide of lead.

2. The fluid, now freed from lead, deposited, on the affusion of ammonia, a greenish matter, which, after ignition, became red, and = 0·033 of a grain. It was oxide of iron.

C.

1. One hundred grains of larger crystals, some of which were hollow, and the surfaces of which were slightly and partially covered with silky filaments, treated in the same way yielded 95·283 of sulphate of lead, equivalent to 69·76 of oxide, and 80 of arseniate of lead, which indicates 26·40 of arsenic acid. The oxide of iron, in this case, amounted to only ·05 of a grain, and the residuary silica was in too small a quantity to be weighed.

2. I have endeavoured to decompose this fossil by boiling it to dryness in a solution of four times its weight of the purest subcarbonate of potash, and exposing the dry mass, for a very short time, to a low red heat; but I found, that only a part of the arsenic acid had united to the alkali; the larger portion of it was detected in the nitric solution of the residuum; but the relative proportions of the oxide and the acid, were found to correspond almost exactly with the foregoing statement of them.

3. I found also, that carbonate of ammonia precipitated this mineral, in an unaltered state, from its solution in nitric

nitric acid: as no arsenic acid had united with the precipitant. The solution of the nitrate of ammonia was evaporated to dryness, and exposed to a red heat in a platina crucible; but nothing was left, except a slight trace of oxide of lead. We may infer from hence, the absence of both the fixed alkalies.

4. I found in one specimen only of this fossil any notable difference in the relative proportions of the oxide of lead and of the acid to which it is united. It consisted of crystals confusedly matted together in a more compact mass than this fossil generally assumes. One hundred grains were dissolved in nitric acid; the marine acid was separated by nitrate of silver, and any redundant silver by muriate of ammonia. The lead was separated by sulphuric acid, and the superfluous portion of that acid by nitrate of barytes, and the arsenic acid was combined with the oxide of lead by the affusion of nitrate of lead. The muriate of silver = 9·8; the sulphate of lead = 97·6, and the arseniate of lead = 72, equivalent to 1·63 of muriatic acid, 71·46 of oxide of lead, and 23·88 of arsenic acid, respectively. The quartz = 0·35, and the oxide of iron 0·2, nearly.

Another portion taken from the same specimen, treated with an alkali, gave very nearly a similar result.

D.

It will now be necessary for me to speak concerning an ingredient of this fossil, which I may have seemed to overlook. I mean the muriatic acid: I have found some difficulty in ascertaining the proportion which it bears to the other constituent parts, and from a cause which I did not suspect. I considered that the only sure mode of determining this point, was to have recourse to nitrate of silver, which might effect a direct separation of the marine acid from the nitric solution of this fossil. But I found, in many experiments upon given quantities of this mineral, that the results, which I derived from this most valuable chemical test, were variable and uncertain.

At last, I was enabled to trace the error and uncertainty up to two sources. In the first place, I found that the muriate of silver was more abundant in the cases where I employed a vessel with a long neck for the solution, and did not expose it to heat.

I concluded, therefore, that when the process was conducted under different circumstances, the predominating

mass

mass of nitric acid produced its effect, and volatilized a portion of the muriatic.

Another source of error I found in the following anomalous circumstance, viz. a simultaneous precipitation of a portion of arseniate of lead takes place with that of the muriate of silver. Whatever combination this may be, it is a weak one, and may be severed by nitric acid, which dissolves the arseniate and leaves the muriate; or by ammonia, which takes up the muriate, to the exclusion of the arseniate.

The conclusion to which many experiments have led me is this, that the muriate of silver produced in the nitric solution of one hundred grains of arseniate of lead by nitrate of silver, amounts to about 9·5.

E.

In order to prove that the acid, which is combined with the oxide of lead in this mineral, is the arsenic acid, and that it is not combined with phosphoric, I decomposed some of its acid, which had been combined with lead in the foregoing experiments, by means of sulphuric acid, and filtered off the sulphate of lead. The fluid which passed through the filter was evaporated nearly to dryness, and it assumed the appearance of crystalline grains. Some of it was exposed to the flame of the blow-pipe in a gold spoon; at first it became like a white dry powder, which melted before an increased heat: placed on charcoal and ignited, it was totally dissipated in arsenical fumes.

Some of it was dissolved in water, and, dropped into liquid sulphate of titanium, a white precipitate was produced: combined with soda, it precipitated silver from the nitrate of silver, of a brick colour. It precipitated mercury from its nitrate, of a yellowish colour, which afterwards became reddish. This precipitate, exposed to the flame of the blow-pipe on charcoal, exhibited the same phænomena as arseniate of mercury.

I precipitated magnesia from its muriate, and redissolved it by carbonate of ammonia, perfectly saturated with carbonic acid. I divided this liquid into two portions, and dropped into both a solution of the combination of the acid of this mineral and soda. No precipitate was produced. I dropped into one of the vessels some liquid phosphate of soda, and a separation of saline matter was instantly produced. I soon, however, found, that this mode of distinguishing the phosphoric from the arsenic acid could not be depended upon. For in the other vessel, in which no phosphate of soda had been dropped, in a short time, saline tufts

tufts made their appearance, and an abundant deposition of saline matter was formed. I found also, that if the solution had been more concentrated, the precipitation would have immediately taken place.

On making a comparative experiment with arsenic acid, I found that it forms a triple salt with ammonia and magnesia, analogous to the phosphoric salt described by Dr. Wollaston. The figure of the arsenical salt, as far as I could determine it from a confused crystallization, is a trihedral prism.

We are therefore, I think, authorized from the experiments herein detailed, to conclude, that the fossil which is the subject of this paper is an arseniate of lead, and that, if we state that the relative proportion of the constituent parts of it is in one hundred, as follows, we shall not be far from the truth:

Oxide of lead	-	69·76
Arsenic acid	-	26·40
Muriatic acid	-	1·58

The silica and the oxide of iron, which account for a portion of the loss, and the alumina and copper which are sometimes found in an analysis of this fossil, I do not conceive to be essential to it.

The existence of a minute portion of muriatic acid as a constant ingredient of it, is a curious fact: and it is still more curious, when we consider it in connexion with the analogy that, in this particular, it maintains with the natural phosphates of lead.

XV. *Description of a reflective Goniometer. By* William Hyde Wollaston, *M.D., Sec. R.S.**

From the advances that have been made of late years in crystallography, a very large proportion of mineral substances may now be recognized, if we can ascertain the angular dimensions of their external forms, or the relative position of those surfaces that are exposed by fracture. But though the modifications of tetrahedrons, of cubes, and of those other regular solids, to which the adventitious aid of geometry could be correctly applied, have been determined with the utmost precision, yet it has been often a subject of regret, that our instruments for measuring the angles of crystals are not possessed of equal accuracy, and that in applying the goniometer to small crystals, where the radius

* From Philosophical Transactions for 1809, Part II.

in

in contact with the surface is necessarily very short, the measures, even when taken with a steady hand, will often deviate too much from the truth to aid us in determining the species to which a substance belongs.

A means of remedying this defect has lately occurred to me, by which in most cases the inclination of surfaces may be measured as exactly as is wanted for common purposes; and when the surfaces are sufficiently smooth to reflect a distinct image of distant objects, the position of faces only $\frac{1}{50}$th of an inch in breadth may be determined with as much precision as those of any larger crystals.

For this purpose, the ray of light reflected from the surface is employed as radius, instead of the surface itself, and accordingly for a radius of $\frac{1}{50}$th of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches; or for greater accuracy we may, by a second mode, substitute the distance of objects seen at a hundred or more yards from us.

The instrument which I use, consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar (Plate II). This axle being perforated, admits the passage of a smaller axle through it, to which any crystal of moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion.

This position of the crystal is first adjusted, so that by turning the smaller axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye.

The circle is then set to zero, or 180°, by an index attached to the pillar that supports it.

The small axle is then turned till the further surface reflects the light of a candle, or other definite object, to the eye; and lastly, (the eye being kept steadily in the same place) the circle is turned by its larger axle, till the second surface reflects the same light. This second surface is thus ascertained to be in the same position as the former surface had been. The angle through which the circle has moved, is in fact the supplement to the inclination of the surfaces; but as the graduations on its margin are numbered accordingly in an inverted order, the angle is correctly shown by the index, without need of any computation.

It may here be observed, that it is by no means necessary to have a clean uniform fracture for this application of the instrument to the structure of laminated substances; for since

since all those small portions of a shattered surface, that are parallel to one another (though not in the same plane), glisten at once with the same light, the angle of an irregular fracture may be determined nearly as well, as when the reflecting fragments are actually in the same plane.

In this method of taking the measure of an angle, when the eye and candle are only ten or twelve inches distant, a small error may arise from parallax, if the intersection of the planes or edge of the crystal be not accurately in a line with the axis of motion*; but such an error may be rendered insensible, even in that mode of using the instrument, by due care in placing the crystal; and when the surfaces are sufficiently smooth to reflect a distinct image of objects, all error from the same source may be entirely obviated by another method of using it.

For this purpose, if the eye be brought within about an inch of the reflecting surface, the reflected image of some distant chimney may be seen inverted beneath its true place, and by turning the small axle may be brought to correspond apparently with the bottom of the house (or with some other distant horizontal line). In this position the surface accurately bisects the angle, which the height of that house subtends at the eye (or rather at the reflecting surface); then, by turning the whole circle and crystal together, the other surface, however small, may be brought exactly into the same position; and the angle of the surfaces may thus be measured, with a degree of precision which has not hitherto been expected in goniometry.

The accuracy, indeed, of this instrument is such, that a circle of moderate dimensions, with a vernier adapted to it, will probably afford corrections to many former observations. I have already remarked one instance of a mistake that prevails respecting the common carbonate of lime, and I am induced to mention it, because this substance is very likely to be employed as a test of the correctness of such a goniometer, by any one who is not convinced of its accuracy from a distinct conception of the principles of its construction.

The inclination of the surfaces of a primitive crystal of carbonate of lime is stated, with great appearance of pre-

* I cannot omit mentioning, that Mr. Sowerby had thought of employing reflection for this purpose, nearly at the same time as myself; but did not succeed to his satisfaction, in consequence of an attempt to fix the position of the eye. For when the line of sight is determined by a point connected with the apparatus, the radius employed is thereby limited to the extent of the instrument; and the error from parallax is manifestly increased.

cision,

cision, to be 104° 28′ 40″: a result deduced from the supposed position of its axis at an angle of 45° with each of the surfaces, and from other seducing circumstances of apparent harmony by simple ratios. But however strong the presumption might be that this angle, which by measurement approaches to 45°, is actually so, it must nevertheless be in fact about 45° 20′; for I find the inclination of the surfaces to each other is very nearly, if not accurately, 105°, as it was formerly determined to be by Huygens *; and since the measure of the superficial angle given by Sir Isaac Newton † corresponds with this determination of Huygens, his evidence may be considered as a further confirmation of the same result; for it may be presumed, that he would not adopt the measures of others, without a careful examination.

IN THE ANNEXED PLATE,

a b. Is the principal circle of the goniometer graduated on its edge.

c c. The axle of the circle.

d. A milled head by which the circle is turned.

e e. The small axle for turning the crystal, without moving the circle.

f. A milled head on the small axle.

g. A brass plate supported by the pillar, and graduated as a vernier to every five minutes.

h. The extremity of a small spring, by which the circle is stopped at 180°, without the trouble of reading off.

i i and *k k.* Are two centres of motion, the one horizontal, the other vertical for adjusting the position of a crystal: one turned by the handle *l*, the other by the milled head *m*.

The crystal being attached to a screw-head at the point *n* (in the centre of all the motions), with one of its surfaces as nearly parallel as may be to the milled head *m*, is next rendered truly parallel to the axis by turning the handle *l* till the reflected image of a horizontal line is seen to be horizontal.

By means of the milled head *f*, the second surface is then brought into the position of the first; and if the reflected image from this surface is found not to be horizontal, it is rendered so by turning the milled head *m*; and since this motion is parallel to the first surface, it does not derange the preceding adjustment.

* Huygenii *Opera Reliqua,* tom. i. p. 73.—*Tract. de Lumine.*

† Newton's Optics, 8vo. p. 329. Qu. 25, concerning Iceland Crystal.

XVI. *Chemical Analysis of a Black Sand, from the River Dee, in Aberdeenshire. By* THOMAS THOMSON, *M.D., Lecturer on Chemistry, Edinburgh**.

THE specimen which formed the subject of the first of the following analyses was brought from the banks of the river Dorr, about seven years ago, by my friend Mr. James Mill, who at that time resided in Aberdeenshire. By him I was informed that considerable quantities of it are found in different parts of the bed of that river,—that it is called by the inhabitants *iron-sand*,—and that they use it for sanding newly written paper. I tried some experiments in the year 1800, in order to ascertain its nature; but was too little skilled at that time, both in mineralogy and practical chemistry, to manage an analysis of any considerable difficulty.

The black powder is mixed with a good many small whitish, reddish, and brownish grains, which, when examined by means of a glass, prove to be pieces of quartz, felspar, and mica. From this it would appear, that the sand of the river Dee consists chiefly of the detritus of granite or gneiss.

When a magnet is passed over the sand, some of the black grains adhere to it, and are by this means easily obtained separate. But after all that can be attracted by the magnet is removed, the greater part of the black powder still remains. This residue is indeed attracted by a powerful magnet, but so very feebly, that it is not possible by means of it to separate it from the grains of sand with which it is mixed. Thus we learn, that the black matter consists of two distinct substances; one of which is powerfully attracted by the magnet, the other not. As this second substance was obviously specifically heavier than the grains of sand with which it was mixed, I placed a quantity of the powder on an inclined plane, and by exposing it cautiously, and repeatedly, to a jet of water, I succeeded in washing away most of the grains of sand, and thus obtained it in a state of tolerable purity.

The first of these minerals we may call *iron-sand*, and the second *iserine*, as they belong to mineral species which oryctognosts have distinguished by these names.

I. *Iron-Sand.*

The iron-sand is much smaller in quantity than the iserine,

* From Transactions of Royal Society, Edinburgh, 1807.

fine, and does not exceed one-fourth of the mixture at most. Its colour is iron-black. It is in very small angular grains, commonly pretty sharp-edged, and sometimes having the shape of imperfect octahedrons. The surface is rough; the lustre is feebly glimmering and metallic; the fracture, from the smallness of the grains, could not be accurately ascertained, but it seemed to be conchoidal. Opake, semi-hard, brittle, easily reduced to powder. Powder has a grayish-black colour; powerfully attracted by the magnet; specific gravity 4·765.

1. As acids were not found to act upon this mineral, 100 grains of it were reduced to a fine powder, mixed with twice its weight of carbonate of potash, and exposed for two hours to a red heat in a porcelain crucible. The mass, being softened in water, was digested in muriatic acid. By repeating this process twice, the whole was dissolved in muriatic acid, except a brownish-white matter, which being dried in the open air weighed 19½ grains.

2. The muriatic acid solution, which had a deep yellowish-brown colour, was concentrated almost to dryness, and then diluted with water. It assumed a milky appearance; but nothing was precipitated. Being boiled for some time, and then set aside, a curdy-like matter fell. It was of a milk-white colour, weighed, when dry, seven grains, and possessed the properties of oxide of titanium.

3. The residual liquid being supersaturated with ammonia, a dark reddish-brown matter precipitated, which being separated by the filter, dried, drenched in oil, and heated to redness, assumed the appearance of a black matter, strongly attracted by the magnet. It weighed 93·7 grains, and was oxide of iron.

4. The 19·5 grains of residual powder, being mixed with four times its weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible, and afterwards heated with muriatic acid, was all dissolved, except about a grain of blackish matter, which was set aside.

5. The muriatic solution being concentrated by evaporation, a little white matter was separated. It weighed one-fourth of a grain, and possessed the characters of oxide of titanium.

6. When evaporated to dryness, and redissolved in water, a white powder remained, which proved to be silica, and which, after being heated to redness, weighed one grain.

7. The watery solution being supersaturated with potash, and boiled for a few minutes, was thrown upon a filter, to

 separate

separate a reddish-brown matter, which had been precipitated. The clear liquid which passed through the filter was mixed with a solution of sal ammoniac. A soft white matter slowly subsided. It was alumina, and, after being heated to redness, weighed half a grain.

8. The brown-coloured matter which had been precipitated by the potash, when dried upon the steam-bath, weighed 20·2 grains. It dissolved with effervescence in muriatic acid. The solution had the appearance of the yolk of an egg. When boiled for some time, and then diluted with water, it became white, and let fall a curdy precipitate, which weighed, when dry, 4·6 grains, and possessed the properties of oxide of titanium.

9. The residual liquor being mixed with an excess of ammonia, let fall a brown matter, which, after being dried, drenched in oil, and heated to redness, weighed six grains. It was strongly attracted by the magnet, but was of too light a colour to be pure oxide of iron. I therefore dissolved it in muriatic acid, and placed it on the sand-bath, in a porcelain capsule. When very much concentrated by evaporation, small white needles began to make their appearance in it. The addition of hot-water made them disappear; but they were again formed when the liquor became sufficiently concentrated. These crystals, when separated, weighed 1·3 grains, and proved, on examination, to be white oxide of arsenic. During the solution of the six grains in muriatic acid, a portion of black matter separated. It weighed 0·2 grains, and was totally dissipated before the blow-pipe in a white smoke. Hence, it must have been arsenic. These 1·5 grains are equivalant to rather more than one grain of metallic arsenic. Thus, it appears, that the six grains contained one grain of arsenic, which explains the whiteness of their colour. The rest was iron. It can scarcely be doubted, that the proportion of arsenic present was originally greater. Some of it must have been driven off when the iron oxide was heated with oil.

10. The insoluble residue (No. 4.) was with great difficulty dissolved in sulphuric acid. When the solution was mixed with ammonia, a white powder fell, which weighed 0·8 grains. It was accidentally lost, before I examined its properties. But I have no doubt, from its appearance, that it was oxide of titanium.

11. Thus, from the 100 grains of iron-sand, the following constituents have been extracted by analysis:

Black

Black oxide of iron, -	98·70
White oxide of titanium,	12·65
Arsenic, - - -	1·00
Silica and alumina, -	1·50
Total,	113·85

Here there is an excess of nearly 14 grains, owing, without doubt, to the combination of oxygen with the iron and the titanium during the analysis.

Had the iron in the ore been in the metallic state, the excess of weight, instead of 14, could not have been less than 30. For the black oxide is known to be a compound of 100 metal and 37 oxygen. Hence, I think, it follows, that the iron in our ore must have been in the state of an oxide, and that it must have contained less oxygen than black oxide of iron. A good many trials, both on iron-sand, and on some of the other magnetic ores of iron, induce me to conclude, that the iron in most of them is combined with between 17 and 18 per cent. of oxygen. This compound, hitherto almost overlooked by chemists, I consider as the real protoxide of iron. Thenard has lately demonstrated the existence of an oxide intermediate between the black and the red; so that we are now acquainted with four oxides of this metal. But the protoxide, I presume, does not combine with acids like the others. Analogy leads us to presume the existence of a fifth oxide, between the green and the red.

As to the titanium, it is impossible to know what increase of weight it has sustained, because we are neither acquainted with it in the metallic state, nor know how much oxygen its different oxides contain. It is highly improbable, that, in iron-sand, the titanium is in the metallic state, if it be made out that the iron is in that of an oxide. The experiments of Vauquelin and Hecht, compared with those of Klaproth, have taught us that there are three oxides of titanium, namely, the blue, the red, and the white. From an experiment of Vauquelin and Hecht, and from some of my own, I am disposed to consider these oxides as composed of the following proportions of metal and oxygen:

	Metal.	Oxygen.
1. Blue,	100	16
2. Red,	100	33
3. White,	100	49

I find, that when the white oxide of titanium is reduced to

 the

the state of red oxide, it loses one-fourth of its weight; and that red oxide, when raised to the state of white oxide, increases exactly one-third of its weight. It was the knowledge of these facts, that led me to the preceding numbers. And I think they may be used, till some more direct experiment lead us to precise conclusions.

Red oxide being the only state in which this metal has yet occurred separate, we may conclude that it combines, in this state, with metallic oxides, and that the titanium in iron-sand is most probably in this state. But white oxide, diminished by one-fourth, gives us the equivalent quantity of red oxide. On that supposition, the titanium present, before the analysis, in the 100 grains of ore, weighed 9·5 grains.

The appearance of the arsenic surprised me a good deal, as it was altogether unexpected. I am disposed to ascribe it to some particles of arsenic pyrites which might have been accidentally present. This conjecture will appear the more probable, when we reflect, that arsenic pyrites very frequently accompanies iron-sand. Before the microscope, the iron-sand appears to contain some white shining particles, which, probably, are arsenic pyrites.

The small quantity of silica and alumina, I ascribe, without hesitation, to grains of quartz and felspar, which had adhered to the iron-sand, and been analysed along with it. Some such grains were actually observed and separated. But others, probably, escaped detection.

12. If these suppositions be admitted as well founded, the iron-sand was composed of

Protoxide of iron,	85·3
Red oxide of titanium,	9·5
Arsenic,	1·0
Silica and alumina,	1·5
Loss,	2·7
	100·0

The loss will not appear excessive, if we consider, that a portion of the arsenic must have been sublimed, before the presence of that metal was suspected.

Upon the whole, I think we may consider the specimen of iron-sand examined, as composed of nine parts protoxide of iron, and one of red oxide of titanium. The presence of titanium in this ore had been already detected by Lampadius, though, as I have not seen his analysis, I cannot say in what proportion.

II. *Iserine.*

II. *Iserine.*

The colour of this ore is iron-black, with a shade of brown. It consists of small angular grains, rather larger than those of the iron-sand, but very similar to them in their appearance. Their edges are blunt; they are smoother, and have a stronger glimmering lustre than those of the iron-sand. Lustre semi-metallic, inclining to metallic. The fracture could not be distinctly observed, but it seemed to be conchoidal; at least nothing resembling a foliated fracture could be perceived. Opake, semi-hard, brittle, easily reduced to powder; colour of the powder unaltered; specific gravity 4·491*; scarcely attracted by the magnet.

1. A hundred grains of the powdered ore were mixed with six times their weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible. The mass obtained, being softened with water, dissolved completely in muriatic acid. When the solution was concentrated, it assumed the appearance of the yolk of an egg. It was boiled, diluted with water, and set aside for some time. A white matter gradually deposited, which, when dried on the steam-bath, weighed 53 grains, and possessed the properties of oxide of titanium.

2. The liquid thus freed from titanium was evaporated to dryness, and the residue redissolved in water, acidulated with muriatic acid. A white powder remained, which, after being heated to redness, weighed 16·8 grains, and possessed the properties of silica.

3. The solution was precipitated by ammonia, and the brown matter which had separated, boiled for some time in liquid potash. The whole was then thrown on a filter, to separate the undissolved part, and the liquid which came through was mixed with a solution of sal-ammoniac. A white powder fell, which, after being heated to redness, weighed 3·2 grains. It was alumina.

4. The brown substance collected on the filter was dried, drenched in oil, and heated to redness. It was strongly attracted by the magnet, and weighed 52 grains.

5. It was digested in diluted sulphuric acid; but not being rapidly acted upon, a quantity of muriatic acid was added, and the digestion continued. The whole slowly dissolved, except a blackish matter, which became white when exposed to a red heat, and, as far as I could judge

* If, as the following analysis would lead us to expect, the specimen examined was a mixture of four parts iserine, and one part quartz and felspar, the specific gravity of pure iserine should be 4·964.

from its properties, was oxide of titanium, slightly contaminated with iron. It weighed 1·8 grains.

6. The acid solution being concentrated by gentle evaporation, a number of small yellowish-coloured needles made their appearance in it. By repeated evaporations, all the crystals that would form were separated. They weighed six grains. I redissolved them in water, and added some ammonia to the solution. A fine yellow powder fell, which I soon recognized to be oxide of uranium. It weighed 4·2 grains.

7. Thus it appears, that the 52 grains (No. 4.) attracted by the magnet contained 46 grains of iron, and six grains of uranium and titanium.

8. The following are the substances separated from 100 grains of iserine, by the preceding analysis :

Oxide of titanium,	54·8
Oxide of iron, -	46·0
Oxide of uranium,	4·2
Silica, - -	16·8
Alumina, -	3·2
Total,	125·0

Here is an excess of no less than 25 grains, to be accounted for by oxygen, which must have united to the three metals during the process. As to the silica and alumina, there can be little hesitation in ascribing them to grains of sand, which had been mixed with the ore. The pure iserine, in all probability, was composed of iron, titanium, and uranium. If we suppose that each of these metals existed in the state of protoxide, we must diminish the titanium by one-fourth, the iron by one-seventh nearly, and the uranium, according to Bucholz's experiments, by one-fifth. This would give us,

Titanium, - -	41·1
Iron, - - -	39·4
Uranium, - -	3·4
Silica and alumina -	20·0
	103·9

Here, then, is still an excess of nearly four per cent. But this I am disposed to ascribe to the oxides of titanium and uranium, having been only dried upon the steam-bath. Upon the whole, it appears, that in the specimens of iserine analysed,

analysed, the proportions of titanium and iron were nearly equal, and that the uranium did not exceed four per cent. The appearance of uranium surprised me a good deal. I perceive, however, that it has already been detected in this ore, from an analysis published by Professor Jameson, in the second volume of his Mineralogy, which, I understand, was made by Lampadius. The specimen examined by Lampadius yielded very nearly 60 parts of titanium, 30 of iron, and 10 of uranium. Whereas, in mine, if the foreign matter be removed, there was obtained, very nearly,

48	titanium,
48	iron,
4	uranium,
100	

But there can be no doubt, that the iserine which I analysed was still contaminated with a good deal of iron-sand; for it was impossible to remove the whole.

XVII. *Analysis of the Gray Copper Ore of Airthrey, in Stirlingshire.* *By* Thomas Thomson, *M.D., Edinburgh* *.

The copper mine of Airthrey, near Stirling, consists of a thin vein, which runs through the west corner of the Ochils. It has been twice wrought, by two different companies; but, in both cases, was abandoned, after a few years' trial. I went to it some years ago, and examined the ore, at the request of one of the proprietors. The specimens which were employed for the subsequent analysis, were the purest that I could select, out of a considerable quantity. I was told, however, that from the lower level, which was at that time full of water, much richer ore had been extracted. But, afterwards, when the lower level was free from its water, I went down to it myself, and found the ore precisely of the same kind as in the upper, with this difference, that it was more mixed with calcareous spar, and perhaps, on that account, more easily smelted.

The veinstones in the Airthrey mine are sulphate of barytes and carbonate of lime, and with these the ore is almost always more or less mixed.

The colour is at first light steel-gray; but the surface soon tarnishes, and becomes of a dark dull leaden-gray,

* From Transactions of Royal Society, Edinburgh, 1807.

and

and in some places assumes a beautiful tempered steel tarnish. Massive and disseminated. In some specimens, it exhibits the appearance of imperfect crystals. Internal surface shining and metallic; but, by exposure, it soon becomes dull. Fracture small-grained, inclining to even. Fragments indeterminate, and rather blunt-edged. Semi-hard, the degree being almost the same as that of calcareous spar; for these two minerals reciprocally scratch each other. Streak similar, opake, brittle, easily frangible; specific gravity 4·878.

1. To free the ore as completely as possible from foreign matter, it was reduced to a coarse powder, and carefully picked. It was then digested in diluted muriatic acid, which dissolved a quantity of carbonate of lime, amounting to 13 per cent. of the original weight of the ore.

2. Thus purified, it was dried on the steam-bath, and 100 grains of it were reduced to a fine powder, and digested in diluted nitric acid, till every thing soluble in that menstruum was taken up. The residue was digested in the same manner, in muriatic acid; and when that acid ceased to act, the residue was treated with nitro-muriatic acid till no further solution could be produced. The insoluble matter was of a white colour; it weighed 6·9 grains, and was almost entirely sulphate of barytes. No traces of sulphate of lead, nor of oxide of antimony, could be detected in it by the blow-pipe.

3. The three acid solutions being mixed together, no cloudiness appeared, nor was any change produced; a proof that the ore contained no silver.

4. The solution being evaporated nearly to dryness, was diluted with water, and precipitated by muriate of barytes. By this means, the sulphuric and arsenic acids, which had been formed during the long-continued action of the nitric acid on the ore, and the presence of which had been indicated by re-agents, were thrown down; for nitrate of lead, added to the residual liquid, occasioned no precipitate; a proof that no arsenic acid was present.

5. The liquid, thus freed from arsenic acid, was mixed with an excess of ammonia. It assumed a deep blue colour, while a brown matter precipitated. It was separated by the filter, and being dried, drenched in oil, and heated to redness, it was totally attracted by the magnet. It weighed 45·5 grains, and was iron.

6. The ammoniacal liquid was neutralized by sulphuric acid; and the copper thrown down by means of an iron plate. It weighed 17·2 grains.

7. To

7. To ascertain the quantity of sulphur and arsenic, 100 grains of the purified ore, in the state of a fine powder, were put into the bottom of a coated glass-tube, and exposed for two hours to a red heat. When the whole was cold, and the bottom of the tube cut off, the ore was found in a round solid mass, having the metallic lustre, a conchoidal fracture, and the colour and appearance of variegated copper-ore. It had lost 16 grains of its weight.

8. The upper part of the tube was coated with a yellowish-brown substance, like melted sulphur. It weighed 12·6 grains. Thus, there was a loss of 3·4 grains. As the tube was long, this loss can scarcely be ascribed to sulphur driven off. I rather consider it as water. For towards the beginning of the process, drops of water were very perceptible in the tube. Whether this water was a constituent of the ore, or derived from the previous digestion in muriatic acid, cannot be determined.

9. When the 12·6 grains of yellowish brown matter detached from the tube were digested in hot potash-ley, the whole was dissolved, except a fine blackish powder, which weighed one grain, and was arsenic. The dissolved portion I considered as sulphur.

10. The potash solution, being mixed with nitric acid, four grains of sulphur fell. The remaining 7·6 grains must have been converted into sulphuric acid, by the action of the nitric acid. Accordingly, muriate of barytes occasioned a copious precipitate.

11. The 84 grains of roasted ore being reduced to a fine powder, mixed with half their weight of pounded charcoal, and roasted a second time in a glass-tube, one grain of sulphur sublimed. But the tube breaking before the roasting had been continued long enough, the process was completed in a crucible. The roasted ore weighed 70 grains.

12. From the preceding analysis, we learn that the constituents of the Airthrey ore are as follows:

Iron	-	45·5
Copper	-	17·2
Arsenic	-	14·0
Sulphur	-	12·6
Water	-	3·4
Foreign bodies		6·9
		99·6
Loss	-	·4
		100·0

If

If we suppose the water and the earthy residue to be only accidentally present, then the only essential constituents are the first four, and the ore would be a compound of

Iron	-	51·0
Copper	-	19·2
Arsenic	-	15·7
Sulphur	-	14·1
		100·0

If we compare this analysis with several analyses of gray copper ore, lately published by Klaproth, we shall find, that the constituents are the same in both; but the proportions of the two first ingredients are very nearly reversed. Klaproth obtained from 0·4 to 0·5 of copper, and from 0·22 to 0·27 of iron. This renders it obvious, that the two ores were not in the same state. I have little doubt, that the difference, however, is merely apparent, and that it arose, altogether, from a quantity of iron pyrites, and perhaps also of arsenic pyrites, which I could not separate from the gray copper ore which I examined. Both of these minerals could be distinctly seen in many of the specimens, intimately mixed with the gray copper; and I have no doubt that the same mixture existed, even in those specimens which were selected as purest. The difference in the proportions of copper and arsenic, obtained by Klaproth * in his various analyses, is so considerable, as to lead to a suspicion, that even his specimens, in all probability, contained a mixture of foreign matter.

XVIII. *Hints on the Subject of Animal Secretions. By* Everard Home, *Esq.*, *F.R.S.* † *Communicated by the Society for the Improvement of Animal Chemistry* ‡.

The brilliant discoveries of Mr. Davy on the powers of electricity in producing chemical changes, suggested to me the

* Gehlen's Journ. vol. v. p. 9, 11, 13.

† From Philosophical Transactions, for 1809, Part II.

‡ Dr. Wollaston's observations inserted in the Philosophical Magazine, were published after this paper had been laid before the Society.

I was led to the present investigation, while preparing my lectures on the Hunterian Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. William Brande to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir Joseph Banks, and stated that I should bring them forward in my lectures;

the idea that the animal secretions may be produced by the same means.

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this society, the object of which is the improvement of animal chemistry. Their intimate acquaintance with these branches of science renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. Davy's discoveries can be applied, and he has given it the consideration it deserves.

The Voltaic battery is met with in the torpedo and electrical eel; and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts: one, that a Voltaic battery can be formed in a living animal; the other, that nerves are essentially necessary for its management; for, in these fish, the nerves connected with the electrical organs exceed those that go to all the other parts of the fish in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a Voltaic battery of high power; but their structure appears to Mr. Davy to adapt them to receive and preserve a small electrical power.

That the nerves arranged with muscles, so as to form a Voltaic battery, have a power of accumulating and com-

tures; at that time Dr. Young's Syllabus was not published, and Dr. Wollaston's opinions were unknown to me.

Dr. Berzelius, professor of chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced:

"Trace all the nerves leading to any secretory organ in a living animal, and divide them, being careful to injure the blood-vessels and the structure of the organ itself, as little as may be: notwithstanding the continued circulation of the blood, the organ will as little secrete its usual fluid, as an eye deprived of its nerve can see, or a muscle whose nerve has been divided can move. We may therefore easily conceive, that any trifling alteration in the nerves of a gland may materially affect its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting, when we discover the insufficiency of all other explanation. Dr. Berzelius's work was shown to me by Mr. Davy while this paper was in the press.

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municating electricity, is proved by the well-known experiment of taking the two hind legs of a vivacious frog, immediately after they are cut off, laying bare the crural nerves, applying one of these to the exposed muscles of the other limb, and then when the circle is completed by raising the other crural nerve with a glass rod, and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs. The ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles, and the course of the nerves of the viscera which keep up a connexion among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

Experiment I. Middle of January, 1809.

The conductors from twenty-four four-inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire was of a deep red colour and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment II. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connexion between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long-continued electrical action, the serum only was employed in the following experiments.

Experiment III. 10th of March, 1809.

The conductors from one hundred and twenty four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment IV. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power: a battery was employed, consisting of twelve four-inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole; in five minutes,

minutes, the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length was seen floating in the fluid, and adhering to the same wire.

Experiment V. 6th of May, 1809.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates very *weakly* charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, (super salts.)

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form; with a less degree, it is separated in a fluid form.

From these facts, the following queries are proposed:

1st. That such decomposition of the blood by electricity, may be as near an approach to secretion as could be expected to be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose; and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed.

5th. That

5th. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6th. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity; whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

XIX. *Geological Remarks and Queries on Messrs.* Cuvier *and* Brogniart's *Memoir on the Mineral Geography of the Environs of Paris. By Mr.* John Farey, *Mineralogical Surveyor.*

To Mr. Tilloch.

Sir, It gave me great pleasure to observe, that you had in the commencement of the present volume of the Philosophical Magazine, introduced the term "*Geology*" into its title; a science, if I may so call it, which stands perhaps more in need than any other, of a respectable channel for freely discussing its facts and principles; and the satisfaction I felt, was much increased, by the perusal of the valuable body of facts which you have therein presented to the English reader, respecting the stratification in the neighbourhood of Paris. We are informed by the very able naturalists who drew up the Memoir alluded to (p. 37), that their geological survey of the environs of Paris (which commenced in 1804 or 1805) was unfinished at the beginning of 1809, but that "some circumstances" compelled them at that moment, to publish an abridged account of their labours, "and to assign *a date*" to their laborious researches, although they were not then brought to a conclusion. What the "circumstances" were which are here alluded to, we are uninformed; but it seems material to the credit of our country, and an act of justice to a merito-

rious Individual to state, that most of the important geological conclusions which our ingenious neighbours on the continent have deduced from their recent survey, were arrived at by Mr. *William Smith*, from an examination of the strata in the environs of Bath, of which he completed a coloured Map, such as M. Cuvier and M. Brogniart describe, and correct Sections also, about the year 1795; and that the same principles and conclusions were in the subsequent years applied and verified, in a general survey made by Mr. Smith, of nearly all the south and east and midland parts of England, and of parts of Wales and Scotland, as many hundred besides myself, who have seen Mr. Smith's Maps and collection of local Specimens, can testify*, as well as the printed accounts which have repeatedly appeared in our newspapers and magazines of Mr. Smith having publicly exhibited these Maps, accompanied by Sections of the British strata, at the Duke of Bedford's and Mr. Coke's sheep-shearings, Lord Somerville's agricultural fêtes, the meetings of the Smithfield Club, the Bath Society, &c. Mr. Smith's extensive collection of specimens, of Organic Remains in particular, which were collected by himself from all the various districts which he visited for forming his Maps, were for several years exhibited to his friends in Bath, and have now for seven years past, been accessible in the same way, at his house, No. 15, Buckingham Street, York Buildings, London.

An opportunity having presented itself in 1801 †, of my studying this subject practically under Mr. Smith, and having since amply verified the truth and ascertained the great value of his discoveries in several Surveys of my own; I ventured, in June 1806, to present the public with a concise sketch of part of Mr. Smith's discoveries, when speaking of the intended Thames Archway, vol. xxv. of your Magazine, and some months after to enlarge somewhat further on these discoveries, in something more than two quarto pages, which are inserted into an article on *Coal*, previously written by some other person, for *Dr. Rees's New Cyclopædia*: to these publications, and those which have followed ‡, I was principally prompted by the solicitations of the friends and well-wishers of Mr. Smith, who,

* The Rev. Joseph Townsend, rector of Pewsey in Wiltshire, was among those who thus profited by Mr. Smith's labours, almost from their commencement.

† This was after Mr. Smith had issued printed proposals for the publication of his work and maps of the British strata, by Debrett, in Piccadilly.

‡ Among these are, *Clay* Strata, *Concentricity* of Strata, *Continent*, *Denudation*, *Excavation* of Valleys, *Extraneous* Fossils, &c., in the Cyclopædia; and a Letter on *Wells and Springs* in the Monthly Magazine for April 1807.

lamenting

lamenting the want of encouragement which suspended his intended publication, foresaw, that at no very distant period, the free communications which he, myself, and others had made, would produce other publications, by which the credit of his discoveries might be lost, not only to himself, but perhaps to our country.

The haste with which the Memoir on the Environs of Paris was drawn up, will be apparent to any one, who shall sit down carefully to digest it, and follow the writers through their details of the interesting geological facts which it contains; in doing which, I found it absolutely necessary to make other arrangements of the Places' names, and of the extraneous Fossils, than those which the writers have presented, as well as to arrange the Strata described, in a Table, to be seen at one view: and thinking that such may prove interesting and perhaps useful to others of your readers, I have transcribed them below, for insertion in your Magazine, in case that you should think them sufficiently suited to the new character which it has assumed.

The difficulty of understanding and following M. Cuvier and his able associate through their details, is greatly increased by the want of a good descriptive Map, such as they promise in their subsequent Memoir; this I have endeavoured to obviate, as well as my very limited knowledge of France and its maps would allow, by giving the bearings, nearly, and direct distances, in English miles, of such of the places named as I could, reckoning from the island in the Seine in the centre of Paris.

Geological students will find an alphabetical arrangement of the *places* where observations have been made, with the strata and fossils annexed; and another alphabet of the *fossils*, with reference to the places and strata to which they belong, highly useful, as companions to the *sections* and descriptions of the strata in their relative positions, and to coloured *mineralogical maps*, showing the surface made by each *soil**; a term which, in the Memoir, answers

* Mr. William Martin, in his useful work "Outlines of the Knowledge of extraneous Fossils," which should be in the hands of every geological student, defines this term, p. 155, and remarks at the bottom of page 158, that we are "to consider the comparative *ages of soils* to be marked, in the first instance rather by *the nature* than *the quantity* of their organic contents," which, though true, is not sufficient to guard us from the Wernerian errors, into which himself has been betrayed in this and some other sections of his work, in maintaining, that the absence of organic remains, marine remains, marine and vegetal, and wood and skeletons, denote the *relative ages* of the substances to which they belong: since experience must teach every one, as it has in part done our Paris mineral surveyors, that *super-position alone* can instruct

answers to an assemblage of contiguous* and allied strata, as the genera of plants and animals do to their species. I shall begin with the names of the material places mentioned, and refer to the pages of the Memoir in your Magazine in each instance: adding occasional remarks of my own, in parentheses.

Antoni, 11 miles SW. by W. of Paris, has plaster quarries at the edge of the gypsous soil; wherein the remains of mammiferæ are found, and whose intervening marles are without lenticular gypsum; probably this is the upper or first mass of the quarryman. p. 53.

Aumont has sand-pits, pure, without fossils. p. 56.

Bagneux, (Bogneux by mistake) 4½ miles SSW., has plaster quarries in the first mass, (see *Antoni* above). p. 53.

Beauce is a very extensive and high platform of (alluvial) Sand, whose extremities are Courville NW., and Montarges SE., (which should have been described in article X. if the memoir had not abruptly terminated) to the westward of Paris, whose very indented north-eastern edge, from the Maulde river to Nemours, forms the south-western limits of the district called the Bason or Environs of Paris, which is the subject of this Memoir. p. 38 and 39.

Bois de Boulogne, 4½ miles NW. by W., is on a plain covered by flint gravel. p. 58.

Bougival near Marly, 10 miles W., has chalk-pits, in distinct beds, with few flints: the chalk here has no covering, except in some places (alluvial) marly sand, containing small and large blocks of calcareous stones of a very fine grain, (perhaps the gray-wethers or cherty blocks of our Wiltshire and other chalk hills). p. 41 and 43.

Champagne Province, to the eastward of Paris, is a very large district of chalk (forming large plains) having isolated patches (or hummocks) of (alluvial) sand upon them. p. 39 and 40.

Champigny, 8 miles SE. by E., has quarries and lime-kilns;

instruct us, as to the relative ages of particular soils. The formations or soils, numerous as they are, which the Wernerians describe in the order of their respective ages, according to his theory, are found inapplicable by most correct observers, in districts distant from those on whose soils they were originally founded; and hence the Derbyshire limestone is not found by Mr. W. Martin to accord with the Wernerian Hypothesis, p. 158: or the gypsum of Paris observed by M. Cuvier and M. Brogniart, with *any* of M. Werner's Formations, p. 53.

* In the order of super-position.

the

the limestone is fine-grained, and was porous (*caverneux*), but is now compact, owing to the infiltration of silex into its cavities; it is without fossils. p. 55.

Chantilly, 17 miles N., has madrepores, with camerines and other well preserved shells, on the declivity of the mountain, and coarse quartz grains, forming a sort of puddingstone. p. 47.

Chatou (Chateu by mistake), 8 miles NW., has flint gravel accumulations. p. 58.

Chaumont, 29 miles NW., is on the north-western edge of the bason or environs of Paris, described in the Memoir; having chalk strata as its boundary; with the coarse limestone strata of *Pallery* in its vicinity. p. 39 and 47.

Chelles has plaster quarries, at the extremity of the gypseous soil, in the first mass, (see *Antoni* above). p. 53.

Clamart, 5½ miles SW., has quarries of coarse limestone, and above these, quarries of plaster; which are at the edge of the gypseous soil, in the first mass, (see *Antoni* above). p. 49 and 53.

Compiegne, 33 miles NE., is at a reentering angle or indentation of the NE. side of the district, called the bason or environs of Paris in the Memoir. Mount Ganelon, with a puddingstone of quartz and shells on it, is near this place. p. 39 and 47.

Concale Bay near St. Maloes, 160 miles W., produced oysters on its shores since the earliest records, which have not given place to other shell-fish. p. 49.

Condé , has pits of gray potters' or plastic clay. p. 44.

Dammartin, 14 miles NE., has plaster-pits in the first mass, which have no covering strata of marle, &c. p. 50.

Espernay (Epernay), 56 miles NE. by E., is at the eastern edge of the bason of Paris, which is described in the Memoir; having chalk strata as its bounds. p. 39.

Etampes, 23 miles SSW., has sand-pits, pure, without fossils. p. 56.

Fontainebleau, 24 miles SSE.——ditto——ditto——ditto.

Fontainebleau Forest, SSE., a rugged and uneven district: siliceous limestone is here found, producing burstones, (French Burs, used here in making mill-stones): sometimes the covering or superficial strata are alluvia or argillaceous marles, and sometimes free-stone without shells, which last alternate with sand, which

is sometimes displaced, leaving the blocks of stone lying in confusion : sometimes these free-stones are coloured or rendered calcareous or argillaceous by infiltration from the covering strata. p. 39 and 56.

Fontenay-aux-Roses, 5½ miles SSW. : here M. Lopez sunk a deep well in his garden, through the gypsum and coarse limestone formations. p. 50.

Ganelon Mountain, near Compiegne, NE., has (alluvial) puddingstone on it, of coarse quartz grains and shells, &c. p. 47.

Gentilly, 3 miles S., has pits of coloured potters' clay, which stratum extends in a range to *Meudon* : it has also quarries of coarse limestone that produce in their lower beds periwinkles, solens, (omitted in the translation) oysters, muscles, pinnæ, calyptræ, pyrulæ, large tellines, terebellæ, porpytes, madrepores, nummulites, and fungites ; above which are strata of green earth with some vegetable impressions, then gray or yellowish strata containing venuses, campreys, and numerous tuberculated cerites : beds of good building-stone ; above these is a stratum containing an entire bed of small long and striated tellines : the fossils are the same in these quarries as at *Grignon* and *Meudon*, and on the top of *Montmartre*. p. 44, 47, 48 and 55.

Gisors, 35 miles NW., is at the extreme NW. corner of the bason of Paris described, and adjoining the chalk districts. Mount *Ouin*, with a puddingstone of quartz and shells in it, is near this place. p. 39 and 48.

Grignon , has quarries of coarse limestone, whose lower beds contain periwinkles, and the twelve other kinds of shells and fossils mentioned at *Gentilly*, with the addition of cerites (as mentioned p. 54) ; also the fossils agree with those of the quarries at *Meudon* and on the top of *Montmartre*. pages 47, 54 and 55.

Grisy is situated at one extremity of the gypsous district and *Meaux* at another ; it has only the first mass of gypsum in its quarries. p. 49 and 53.

Guespelle , the strata of this place answer to those of *Gentilly*, above. p. 47.

High Normandy province, to the north-west of Paris, is wholly composed of chalk (forming the NW. border of the basin of Paris). p. 39.

Houdan, 28 miles W., has pits of gray potters' or plastic clay. p. 44.

Issy, 4 miles SW. by W., has quarries of coarse limestone,

stone, with periwinkles and the twelve other fossil remains in their lower beds, that are mentioned of *Gentilly*. p. 47.

Láonnois is a district (of chalk) NE. of *Compiegne* between the rivers Oyse and Aure, which bound the basin of Paris, and forms therein a considerable reentering angle. p. 39.

Longjumeau, 9 miles SW. by W., has siliceous freestone and sand, containing the 13 kinds of shells, &c., found at *Gentilly* and *Grignon*, with the addition of balani: towards the top of the hill a sand (probably alluvial) contains lymneæ and planorbis with siliceous wood, and other parts of vegetables. p. 54 and 57.

Mantes, 22 miles WSW., is on the NW. edge of the basin of Paris, and adjoins the chalk district without it. p. 38 and 39.

Maulde River, which empties itself into the Seine, is at that place the limits of the basin of Paris, at the NW. end of the (alluvial) sands of *Beauce*, which form its crooked boundary SE. thence to *Nemours*. p. 38 and 39.

Meaux, 19 miles NE. by E., is situate at one extremity of the gypseous district within the basin of Paris, and *Grissy* is at another. p. 49.

Meudon, 6 miles SW., has chalk-pits, in which are layers of flints about 6½ feet apart, but no detached flints in its mass; the upper part of the chalk is in a rubbly state, with the clay of the superior strata in its interstices. A stratum of coloured potters' clay, without fossils, covers the chalk here, and extends south-eastwardly to *Gentilly*: above this clay, coarse limestone strata here occur, and the lower beds in the quarries contain calyptreæ, fungites, madrepores, muscles, nummulites, oysters, periwinkles, pinnæ, porpytes, pyrulæ, solens, large tellines, and terebellæ; which fossils are of the same kinds as those of the quarries of *Gentilly* and *Grignon* and on the top of *Montmartre*. The gypsum soil occurs here upon the coarse limestone soil; there are only thin beds of plaster in it, but which are sufficient to fix the relative super-position of these soils. pages 41, 43, 44, 47, 49, 53 and 55.

Montmartre, 2½ miles N., has had its strata and fossils described by M. Desmarets; it has large plaster quarries in which are three principal masses of gypsum, the lowest of which is without fossils, and has only thin beds of gypsum, frequently selenitous, with intervening strata

of solid calcareous marles, that contain coarse crystals of lenticular yellowish gypsum; and with very scaly argillaceous marles that contain menilite silex. The middle mass has thicker and more numerous beds of gypsum, the intervening solid calcareous marles have in them numerous coarse crystals of lenticular yellowish gypsum: some of these beds, those that are marbled with gray and are compact and argillaceous, are used for scouring-stones (not building). It contains scattered nodules (*rognons*) of sulphated strontian in its lower beds; and it is chiefly in this mass that fossil fish (not shell-fish) are found.

The upper mass is very thick, chiefly of beds of gypsum, interlaid with a few marley strata: its lower beds of gypsum are intermixed with siliceous masses, its middle beds are columnar, and its upper gypsum beds are usually interlaid with five strata of marle: this upper gypseous mass contains astonishing collections of the skeletons of birds, of quadrupeds, mammiferæ, and fish, tortoise bones, and a few shells, agreeing with those of the fresh-water fish of our lakes. A large mass of marles occur above these, imbedding different fossils (for which I must refer to page 51 and 52, to the Section, and to what follows herein).

Above these marles, freestone and sand here occur, that contain the 13 kinds of fossil remains, mentioned at *Gentilly*, *Meudon* and *Grignon*, with the addition of balani shells. pages 37, 50, 51, 52, 54 and 55.

Montmorency, 10 miles NE., has the upper mass of gypsum, and its fossils (see *Montmartre*) forming the surface, in some parts; in others, it has above the gypsums and marles, the freestone and sand which contain the 14 kinds of fossil remains that are found on *Montmartre* &c. p. 50 and 54.

Montereau, 22 miles SE., is at the south-eastern edge of the basin of Paris described by our authors, and adjoins the great chalk districts in *Champagne*: gray potters' clay is here dug, upon the chalk. p. 39 and 44.

Montfort, 18 miles W., is near the western edge of the basin of Paris. p. 39.

Mont Valerian , has plaster quarries, near the edge of the gypsous district, where only the first or upper mass of gypsum appears. p. 53.

Moret, in Dreux Forest, has pits of very white potters' (or pipe-)clay; it covers the chalk, and is without fossils. p. 44.

Nanterre,

Nanterre, $7\frac{1}{2}$ miles NW., has a plain covered by flint-gravel. p. 58.

Nemours, 32 miles SSE., is at the limits of the basin of Paris, and at the SE. end of the (alluvial) sands of *Beauce*, which form its crooked boundary thence NW. to the mouth of the *Maulde* river. p. 39.

Neuilly, $4\frac{1}{2}$ miles NW., has quarries of stone, in which are found crystals of quartz and rhomboidal crystals of variegated carbonate of lime, but no fossil remains. p. 48.

Ouin Mountain, near Gisors, NW., has (alluvial) pudding-stone of coarse grains of quartz and shells. p. 48.

Palaiseau , has strata of freestone, without shells, alternating with sand, which is sometimes washed out or displaced, and the blocks of stone are left in confusion. p. 56.

Pallery, near Chaumont, has in its lower strata of coarse limestone and sand, the thirteen kinds of fossil remains that are found at *Gentilly*. p. 47.

Paris Environs has had its stratification and extraneous fossils treated of by M. Desmarets, M. Gillet-Laumont, M. Lamarck, M. Coupé, and M. Cuvier. About the beginning of 1805, M. Cuvier and M. Brogniart commenced a Mineralogical Map of that district around Paris, denominated by them the *Basin of Paris*, in which the chalk in horizontal beds with flints, (being the lowest stratum there known) is wholly or in part covered by certain argillaceous, siliceous, calcareous, gypseous and alluvial strata; the nature, contents and relative situations and thicknesses of which, it is the object of their present labours to investigate and explain. This basin, measuring directly from *Espernay* to *Gisors*, nearly from east to west, is 87 English miles long, and from *Nemours* to the neighbourhood of Noyon (to which it must extend to form the reentering angle that is mentioned p. 39), nearly south and north, it is 70 miles broad. This noble and extensive field for geological investigation, has on all sides natural limits, which seem very crooked and indented, as might be expected: on the SW. from near *Nemours* to the mouth of the *Maulde* river (a direct distance of about 45 miles), it is limited by a covering stratum of Beauce *sand* (which I suppose from its description to be alluvium), and on all its remaining sides

sides it is said to be bounded by the naked chalk-stratum above mentioned*. The Seine river enters the basin of Paris near *Montereau*, and quits it again near *Gisors;* its tributary rivers the Loing enters it near *Nemours*, the Marne at *Espernay;* and the Aune and Oyse near *Compiegne*, after having for some distance skirted its boundaries. pages 37, 39, 40 and 41.

Picardy Province, to the north of Paris, is wholly composed of chalk (forming the northern border of the basin of Paris), having isolated patches (or hummocks) of sand upon it, (perhaps alluvia). p. 39 and 40.

Rolleboise, on the banks of the Seine, has in the plastic or potters' clay, fragments of bituminous wood: this clay covers the chalk, and underlays the coarse limestone. p. 44.

Romainville, 4½ miles NE., has the marley strata that cover the gypsum beds; and in a white and friable calcareous stratum there, siliceous trunks of palm-trees of large bulk, and lymneæ and planorbes shells are found, similar to the species that now exist in our marshes: at this place also freestone and sand are found that contain the 14 kinds of fossils mentioned at *Grignon*, and also balani shells. p. 51 and 54.

Saint Cyr, 14 miles WSW., has the gypsum formation, but no beds of plaster; green marles accompanied by strontian here prevail. p. 53.

Saint Germain, 12 miles NW., has coarse limestone quarries and their accompanying fossil remains (as at *Gentilly*), and above these is a soft green bed with marks of leaves and stalks of vegetables in its lower strata: on this a freestone rock containing roundish venuses, campreys and numerous tuberculated cerites, and above that a thin hard stratum whose seams abound with small long white striated tellines. p. 48.

Saint Germain Forest has certain parts of it covered by flint gravel. p. 58.

Saint Prix, 11 miles NE., has free-stone and sand strata, that contain the 14 kinds of fossils mentioned at *Grignon*, and also balani shells. p. 54.

Seran, on the canal of Ourque, has a deep excavation, made

* Which seems the same that once extended across the Channel to the British shore, where it is now found, with local interruptions, from the Isle of Wight to the mouth of the Thames. See vol. xxxiv. p. 310.

in

in a marshy bottom for the canal, in alluvial soil, where the bones of elephants and large trunks of trees were found. p. 58.

Sevres, 7 miles W., has quarries of coarse limestone and sand, in which are found the 13 fossil remains mentioned at *Meudon*. Near the glass-house, the mass of chalk (that underlays the above), is elevated near 50 feet above the Seine, and is apparently the highest part of it in the basin of Paris; the strata upon this elevated part seem thinner than usual, and the stone is sensibly inclined towards the Seine, (probably these appearances are occasioned by a fault and consequent tilt of the strata, as these are the only inclining strata that are mentioned in all the basin of Paris) p. 43 and 47.

Sezanne, 48 miles SE. by E., is at the south-eastern edge of the basin of Paris, adjoining the extensive plains of chalk in *Champagne*. p. 39.

Trappe, 14 miles SW., has hard siliceous limestone strata, which quickly perish on exposure to the air and rain; and such is here used for marling the land: in these strata burstones (French Burs) are found, with lymneæ and planorbes shells and gyrogonites, supposed to be of fresh-water origin. p. 56 and 57.

Triel, , has plaster quarries on the mountain, that are situate at an extremity of the gypsous district, in which only the first mass of gypsum is seen (as at *Antoni*): beneath which are strata and quarries of coarse limestone (and its accompanying fossils as at *Gentilly*). p. 49 and 53.

Vaucienne Valley has madrepores, with camerines and other well preserved shells, and grains of coarse quartz, that together make a sort of puddingstone. p. 47.

Vaugirard, 2½ miles SW. by W., has quarries of coarse limestone, with calyptræ and the 12 other kinds of fossil remains in their lower beds, that are mentioned at *Meudon*. p. 47.

Versailles Park, 14 miles SW., is situated in a strait between two hills (or in the inosculation of two opposite valleys) where the gypsum formation has no plaster beds in it, but green marles containing nodules of strontian prevail. p. 38 and 53.

Ville-d'Avray, 8 miles W., has plaster quarries in some places, and in others only thin strata of plaster in the gypsum formation; beneath which are strata and quarries of coarse limestone (and its accompanying fossils as at *Gentilly*). p. 49 and 53.

Villepreux,

Villepreux , has coarse limestone quarries, and their accompanying fossil remains, (as at *Gentilly*) with the covering strata such as occur at *Saint Germain*. p. 48.

Villers-Cotteret, 31 miles NE., has madrepores, with camerines and other well preserved shells, and grains of coarse quartz, that together make a sort of puddingstone. p. 47.

Viroflay, 9¼ miles SW. The gypsum formation has here no plaster beds in it, but green marles containing strontian prevail. p. 53.

An Alphabetical List of the Fossils *or Organic Remains, which are mentioned by Messrs. Cuvier and Brogniart as found within the Basin of Paris; with the Names of the Places, and of the Strata which produce them, with References to the Pages in the Philosophical Magazine, and Remarks in parentheses.*

Ananchites (Echini?), the shells of calcareous spar, filled with black flint, in the chalk strata. p. 42.

Anomites, three Terebratula shells in the chalk strata. p. 42.

Antelopes, bones of unknown species, in the alluvium (of valleys). p. 58.

Balani shells, in the freestone at top of Montmartre, at Romainville, Saint Prix, Montmorency, Longjumeau, &c. p. 54.

Belemnites, in the chalk strata; these differ from those found with ammonites in compact lime*. p. 42.

Birds, skeletons of unknown ones, at Montmartre, &c., in the first or upper gypsum mass. p. 51.

Calyptræ shells, at Chaumont, Gentilly, Grignon, Guespelle, Issy, Longjumeau, Meudon, Montmartre, Montmorency, Pallary, Romainville, Saint Prix, Sevres, Vaugirard, &c. in the lower beds of the coarse limestone, and in the freestone. p. 47, 54, and 55.

Camerine shells, at Chantilly, Ganelon mount, Ouin mount, Vaucienne, Villers-Cotteret, &c., either in the lower beds of the coarse limestone, or in a siliceous puddingstone. p. 47 and 48.

* The limestone here spoken of, does not seem to belong to the basin of Paris, and I should be very glad to learn, whether it is any-where dug in France? In the great argillaceous formation between the Bath Freestone and the blue Lias soils, in this country, there is a stratum of limestone called by Mr. Smith the blue marle-stone, which contains very perfect belemnites, small cornu-ammoni and asteria. The limestone of Oakham, and of Maidwell, between Northampton and Market-Harborough, belongs I believe to this stratum, but I never had the opportunity of collecting any specimens at the latter place.

Camprey

Camprey shells, at Gentilly, Saint Germain, Villepreux, &c. in the building-stone rock above the coarse limestone. p. 48.

Cardium shells are found in the yellow argillaceous marle, some distance above the green potter's earth of the gypsous soil, near its top. p. 52.

Caryophyllæa (a polypier) in the chalk strata. p. 42.

Cerite, shells, often in fragments, at Gentilly, Grignon, Meudon, Montmartre, &c., in the lower beds of the coarse limestone, and in the freestone. p. 54 and 55.

———————, tuberculated, at Gentilly, Saint-Germain, and Villepreux, &c. in great numbers in the building-stone rock, above the coarse limestone. p. 48.

———————, often in fragments, in the yellow argillaceous marle near the upper part of the gypseous formation. p. 52.

Cranium, or skull, of some animal, in the chalk strata. p. 42.

Elephants' bones of unknown species, at Seran, &c. in the alluvium (of valleys). p. 58.

Fibrous shells, resembling very thick pinnæ, in fragments, in the chalk strata. p. 42.

Fish (not shell-fish), at Montmartre in the marbled gray scouring (not building) stone of the middlemost gypsum mass, having scattered nodules (*rognons*) of sulphated strontian in its lower beds. p. 50.

——— skeletons, at Montmartre, Dammartin, Montmorency, &c., in the gypsum or intervening marles of the upper gypsum mass or first of the quarrymen. p. 51.

Fresh-water shells (resembling recent species of such shells) Lymneæ and Planorbes, at Montmartre, Romainville, &c., in the upper gypsum mass, or the marles which cover it; and from the decomposing stone at Trappe, near Versailles, (denominated fresh-water by our Authors) resembling siliceous limestone, when first dug. p. 51 and 56.

Fungites, at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sèvres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Geodes, round stones, some hollow, of a very fine-grained limestone, containing multitudes of small spiral univalves; whose cavities contain a new variety of crystallized sulphate of strontian, called apotamous-sulphated strontian; these geodes are found in a marley (alluvial) sand, in some places, at Bougival near Marly, covering the chalk strata, which are themselves not found

found to contain any simple and regular spiral univalves. p. 42 and 43.

Gyrogonites, small round hollow (furrowed) bodies, resembling no known recent body, found at Trappe near Versailles, in the decomposing soil (denominated fresh-water soil by our Authors) which, when fresh dug, resembles hard siliceous limestone. p. 57.

Leaves of vegetables and stalks, at Gentilly, Saint-Germain, Villepreux, &c., brownish, in the lower beds of the green earth, that underlay the building-stone rock of the coarse limestone soil. p. 48.

——— and parts of vegetables are found, changed into silex, near the top of the hill at Longjumeau, in (alluvial) sand. p. 57.

Lituolites of two species, in the chalk strata. p. 42.

Lymneæ shells agreeing with recent species, at Romainville, in a white and friable calcareous mass above the gypsums; and at Trappe near Versailles, in a marle there used in agriculture, which when fresh dug resembles hard siliceous limestone. p. 51, 56 and 57.

——— coarse shells in silex, are found in the (alluvial) sand, on the tops of the hills at Longjumeau. p. 57.

Mactre shells are found in the yellow argillaceous marle that is above the thick green potters' earth of the gypseous formation. p. 52.

Madrepores, at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Mammiferæ, bones of, at Antoni, Bagneux, Chelles, Clamart, Grisy, Montmartre, Triel, Valerain-mont, &c. in the upper gypseous mass. p. 51 and 53.

————— skeletons, of large unknown kinds, are found in the putrid alluvium (of some of the valleys) of the basin of Paris. p. 58.

Millepora, (a polyper) of decomposing pyrites, in the chalk strata. p. 42.

Mollusci of the chalk strata are quite different from the testaceous mollucsi of the strata above the chalk. p. 54.

Muscles of one particular species are found in the chalk strata, p. 42.

——— at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone, are found to be of other species. p. 47.

Num-

Nummulites are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c. in the lower beds of the coarse limestone strata. p. 47.

Oxen (bulls or cows), skeletons of unknown species, in the alluvium (of valleys). p. 58.

Oysters of two different species are found in the chalk strata. p. 42.

——— at Chaumont, Gentilly, Grignon, Guespelle, Issy, Longjumeau, Meudon, Montmartre, Montmorency, Pallery, Romainville, Saint Prix, Sevres, Vaugirard, &c. Other kinds of oysters are found in the lower beds of the coarse limestone, and also in the freestone. p. 47, 54 and 55.

——— are found in the marles that are immediately below the argillaceous sand, at the top of the gypseous formation, in two beds, the lower containing large, and the other small oyster shells; they are met with over all the district wherein the entire gypseous formation is found. p. 52.

Palm-trees, trunks of large size, are found converted into silex, lying along in a whitish friable calcareous mass above the gypsums. p. 51.

Periwinkles are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Pinnæ shells of a particular species are found in the chalk strata; in which also, fragments of thick shells of a fibrous structure are found, which our Authors consider as more like pinnæ than any other known genus. p. 42.

——— of different species are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Planorbis shells, analogous to the recent species of lakes and marshes, are found at Romainville, in a white and friable calcareous mass above the gypsums; also, at Trappe near Versailles in the husbandry marle, which though very decomposing, resembles a hard siliceous limestone when fresh dug. p. 51, 56 and 57.

——— coarse siliceous shells are found at the tops of the hills at Longjumeau, in a sand (which is probably alluvial). p. 57.

Polypiers of five or six different sorts are found in the chalk strata. p. 42.

Porpytes

seous formation; which thin seam can be traced in the quarries, over a space of $27\frac{1}{2}$ miles long and 11 miles broad. p. 52.

Terebellæ are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Terebratula, see Anomites.

Thornback fish, fragments of the bones of, found in the yellow argillaceous marle, above the thick green potters' earth of the gypsous formation. p. 52.

Tortoise bones are found at Dammartin, Montmartre, Montmorency, &c., in the gypsum or intervening marles, of the upper gypsum mass. p. 51.

Trees, trunks of, large palms (apparently), converted into silex, are found lying flat in a white and friable calcareous stratum, above the upper gypsum mass. p. 51.

——, large trunks of, of unknown species are found at Seran, &c., in the putrid alluvium (of the valleys). p. 58.

—— see Wood.

Trochi shells are found in the yellow argillaceous marle, near the upper part of the gypsous formation. p. 52.

Vegetables, see Leaves.

Venus shells, roundish, are found at Gentilly, Saint-Germain, Villepreux, &c., in the building-stone rock of the coarse limestone formation. p. 48.

————, sometimes in fragments, are found in the yellow argillaceous marle near the top of the gypsous formation. p. 52.

Vermiculites, of three species, are found in the chalk strata. p. 42.

Wood, bituminous, at Rolleboise, by the Seine river, in the potters' or plastic clay formation, in which no other organized remains have been found. p. 44.

—— siliceous or petrified, at Longjumeau, on the tops of the hills in (alluvial) sand. p. 57.

——, see Trees.

In order to supply, in some degree, the want of vertical Sections across the basin of Paris, in some determinate directions, best adapted for exhibiting the thicknesses and relative positions of its several strata; (which sections, it is to be hoped, will accompany the promised Memoir of M. Cuvier and M. Brogniart); I have, in the mean time, attempted an arrangement from their abridged Memoir, which, perhaps, you may deem worth a quarto page, to enable it to be folded out at the end of this Letter. In this table, I

 have

have separated the IXth and Xth soils or formations by a double line, to mark the distinction between them and the remaining eight soils; which our authors seem to consider as composed of regular or undisturbed strata, that is, such as exhibit no marks of violent attrition or mechanical mixture, as the alluvial soils almost invariably do; and they, besides, never underlay regular strata.

I shall now, I hope, be excused in offering freely, a few of the observations which have occurred to me, in thus analysing the hasty but valuable Memoir of our able and industrious neighbours on the continent; and comparing it with what I know of the terrestrial stratification, and which is indeed limited in a great measure to the British Islands.

England presents us with the opportunity of examining the out-crop, or appearance on the surface, of a vast succession of strata, among which the fourth, or lowest Derbyshire limestone, is perhaps three miles of perpendicular depth, below the part of the chalk strata, which are described as the lowest that have been seen or reached in the basin of Paris: for it was ascertained by Mr. *William Smith* several years ago, that 200 feet or more of the lower part of the chalk strata are entirely without layers or nodules of flint; and that some of these beds contain large cornu-ammonis, nautili, and numerous other shells: this he has proved by an actual examination of very near 700 miles in length, of the basset or out-burst of these lower and hard beds of chalk, without flints, reckoning from the Isle of Wight to the westernmost points of the chalk in Dorsetshire and Somersetshire, and thence for the north-east angle of Norfolk; through Lincolnshire, and Yorkshire to Flamborough-Head; and including the edges of our great southern denudation * from Beachy-Head in Sussex, to near Petersfield and Alton in Hampshire, and Farnham in Surry; and thence again to Hythe in Kent. And here it may not be amiss to observe, that before I knew Mr. Smith, he had ascertained the peculiar fossils that belong to several of the thin strata, underlaying the whole extent of the edges of this vast mass of chalk, in the same manner as the Paris strata overlay it, had collected a series of specimens of each fossil, marked and brought together from as many places; and was well entitled, and did in fact often exclaim, though perhaps, in

* This denudation is shortly mentioned in the Cyclopædia, article *Coal*, and is further exemplified by a manuscript section of the strata between London and Brighton, made by me in 1806, which is in the hands of many.

other

other words: "This constancy in the order of superposition of the thinnest strata, and over an extent of" 700 miles in length, "is one of the most remarkable facts which" I "have established. From this, there ought to result most interesting consequences to the Arts, and to Geology."

But let us return to our chalk strata, which, as they form the basis of the Paris observations, require some further notice. I am myself, unfortunately, not sufficiently acquainted with the French coast, to state the exact point where the chalk terminates on the shore to the south of Bologne: and whether its termination there is occasioned by its edge rising, passing inland, and exhibiting the under strata on the coast, or whether the chalk by sinking down permits its upper strata to cover it at the level of the shore*; the former should perhaps seem the most probable, from the circumstance of M. Cuvier and his associate being able to assign a limit of naked chalk on all the north side of the natural district, which they have rendered so interesting to science, under the appellation of the basin of Paris. On the north-east of Calais, the great plain of chalk seems to decline northward, and either its upper strata or alluvial matters, of no immeasurable thickness I apprehend, progressively cover it as we proceed along the coast, or across any part of the Netherlands, from France to the southern part of the coast of Denmark. The chalk having, somewhere in this immense flat, obtained its lowest point, as the bed of the German Ocean, it begins to rise again northward, finally to terminate in the Dogger-Bank and the other shoals which are seen in good charts, stretching across from the coast of Yorkshire to the Danish coast, where the chalk rises again, and its northern edge passes inland and across the island of Zealand, as I have been informed, the coast northward becoming occupied by the lower strata, which also pass over from the Yorkshire coast, further north than Flamborough Head. How then does it happen, that this vast *plain* of chalk (which it still is, if we neglect its local irregularities) extending from Normandy to Denmark, from the confines of Hanover to Dorsetshire, and from the southern border of Champagne to the middle of the Yorkshire coast, exhibits the strata and astonishing organic remains of the basin of Paris, only within that spot? which is small compared with the whole space in which the chalk lies buried under other strata, either regular or alluvial. Is it

* Perhaps some of your correspondents can gratify me and the other geological inquirers among your readers, by an answer to these questions.

 because

because the edges and accessible parts of the strata superincumbent upon chalk, have not been sufficiently till now any where explored, to detect the peculiar and interesting indications of these strata, which we have now the authority of two most able naturalists, in saying, exist in such profusion, and are each disposed in such unerring regularity, that a very thin bed of telline shells was observed by them over a district more than 27½ miles long, and more than 11 broad? This I think would hardly seem likely, as far as Great Britain is seated upon the chalk strata, after the long and indefatigable researches of Mr. Smith and of his followers, as well as of other English naturalists, who seem always to have been alive to preserving and describing the bones, teeth, and other rare fossil remains, which accident or the miner exposed; though in few instances, perhaps, with all these acute discriminations, which our authors are able to bring into this important field of inquiry. Again, is it because the Paris strata, though deposited alike on the English chalks, either never did here hold, or now preserve no traces of the myriads of organized remains, which are so perfectly preserved in the French part of these strata, although they once existed here also? And again, are the London clays, and the sands beneath and above them (in some places), to be considered as the Paris strata in a modified state? * Or, should the Paris strata be wholly wanting in England, in that case, are the matters here covering the chalk, belonging to the alluvium? or are they local strata placed by the side, or parallel with, the Paris strata, as our authors maintain to be the case with two of their soils or formations, viz., the Vth and VIth?

I shall only offer a few remarks, and not attempt, at present, a full discussion of the above six queries, but beg rather to throw them out, as subjects worthy the researches and consideration of your readers and correspondents, and particularly of the members of the London Royal and Geological Societies, who will, I hope, excuse me in hinting, that the national honour seems to call upon them, to bestir themselves, in the accurate investigation of the strata on which they are seated: and in the discussions necessary for truly fixing their relations to those, on which the Parisian Institute have thus lamentably been suffered to take the lead.

It will be gathered from the imperfect sketches, to which

* Perhaps as great dissimilarity in the state and appearance of some particular strata, in distant places, has in other instances been observed in Britain, where the inferior and superior strata prove the identity.

I have

I have referred, as well as recollected by Mr. Smith's pupils and acquaintance, that he considers and teaches, that the western edges of the several strata on the south and eastern parts of England, are their natural *endings*, and that such strata never did proceed further towards the north-west than they do at present, and that all the strata are not entire or perhaps ever were, some having large and deep holes or patches wanting within their limits, as well as in their deeply fingered or indented edges; while other strata, particularly about Bath, have detached parts beyond their continuous mass forming hummocks, or capping the adjacent western hills. These facts or appearances of the strata have accorded, generally speaking, with the several English districts which I have of late years examined; but many of the minuter circumstances attending the holes or denudated patches in the strata, which have been more particularly the object of my researches, such for instance as the strata around and under these denudations, *rising on all sides* towards some central point or ridge; their edges being so often abrupt and straight, like fractures rather than the rounded endings of strata; these denudations, as far at least as observations extend, being all elongated in a marked manner from SE. to NW., or very nearly so; and others, besides those which I have elsewhere mentioned as the characters of *denudation*, and above all, the vast extent to which I find these denudations extending, in the southern and middle parts of England, have occasioned me to hesitate considerably, as to the real distinctions, between the natural endings and the denudated edges of our strata: and it should seem, that the idea so natural and general among neptunists, of the strata having been originally level and *concentric*, may, by extending the magnitude of the denudations (to which no limits have yet been assigned), account for all the external forms of the English strata, and perhaps for the admirable form of its valleys, although I may again invite the sarcastic lash of a certain class of writers, in frankly owning that I am unable to guess, much less to describe the mechanical operations by which this has been brought about; and in referring it *to the finishing and all-wise operations of Creative power, on the mass of the earth,* or old world, as our authors have denominated it; which had been the theatre of such a long and astonishing series of the creation and extinction of animated beings, during the accumulation of its matter, for purposes which are perhaps to us inscrutable.

Whether the isolated hillocks or hummocks of gypsous

and other strata, which our authors have described in the basin of Paris, are the detached endings of similar and connected strata, that lay to the south and west of Paris, and may either be concealed by vast alluvial tracts (p. 40), or may occupy countries where nice observations have not yet been made; or, whether such hummocks be occasioned by irregular or compound denudation*, such as I have noticed several instances of in Derbyshire, and as Mr. Smith has observed about Bath, and Dr. William Richardson in the north of Ireland †, it is of course difficult to determine, and its discussion must be deferred, until a numerous train of facts are better understood.

Our authors, it will be seen, recur to a local inundation or lake of *fresh-water* ‡ for explaining the deposition of the matter composing their gypsous hillocks: to me, however, much better evidence seems wanting, of the probable existence of such a *lake* or local pool of fresh-water than they offer, and particularly of the several successive *fresh-* and *salt-water* inundations which they mention, at pages 54 and 55. Has it, or can it be proved, that fish nearly or exactly resembling in the genera, or even the species, those that are *now* peculiar to either *fresh-* or *salt-water*, may not have had other powers and habits in the *old world?* and that all the animals and even birds hitherto known of the primitive creation, and its vegetables also, may not have been adopted by their great Author, to a sub-aqueous existence? And again, Who has or perhaps can, determine, whether the fluid surrounding the earth during its formation which I have called aqueous above, was either fresh or salt, in the sense that we now use those terms? Does it not seem more probable, that this fluid varied essentially from time to time in its nature and composition, and which perhaps occasioned the extinction of the beings adapted to its prior state, as well as the alternations of the strata, especially, if the matters of the strata were ever in chemical solution in it, as our authors seem to imply (p. 51), in

* Perhaps the basin of Paris may lie within the southern verge of the great south-eastern denudation of England, that reaches far into Hampshire, and has apparently stript off all the chalk strata, from near Alton to Bologne on the French coast.

† See our 33rd volume.—Edit.

‡ It seems improbable that gypsum should be a produce of fresh-water, when we reflect, that the red soils or gypsous marles of England produce both sea salt and gypsum in vast abundance, and that salt rocks and springs are the common accompaniments of gypsum in various other parts of the world; we are not told, whether there are any brackish or salt springs within the basin of Paris.

speaking

speaking of the gypsums being crystallized from the waters of their Parisian lake?

The candid confession of the essential differences between the *seas*, the *fresh-waters* and the *alluvium* of the old world, and the seas, fresh-waters and alluvia of the present world, made by our authors, in the concluding paragraphs of their IIId, VIIIth, and IXth articles, do them credit, and give some weight to my suggestions above, which are not here made for the first time.

Our authors say (p. 37), that "a strongly marked character of a great eruption proceeding *from the south-east* is imprinted on the forms of the eminences," on which subject it may be proper to remark, that soon after Mr. Smith had commenced his investigation of the British strata, he discovered an important law regulating all the *known alluvia*, or that which consisted of or contained the fragments and reliquia of known strata, were moved *from the south-east* towards the south-west: the matters of any particular stratum being rarely if ever found as alluvium upon that stratum, but such matters are found more or less plentifully on the surface, beyond its western edge: instances are numerous in England of considerable and unbroken masses of soft or clayey known strata, being moved many miles and lodged in the alluvia; stones of large size thus moved, and scarcely at all rounded, are exceedingly common in some districts, and such are often lodged on the highest hills, but the most common state of the native alluvia, is in small water-worn and mixed fragments: numerous hills in Bedfordshire of considerable extent are thus formed or raised higher, by alluvia of the chalk strata and those which cover it, including a vast variety of limestone bolders that are full of shells, many of which, if properly examined, would I suspect agree with the coarse limestones of the basin of Paris: my researches in the midland counties, have detected many isolated patches of exactly similar alluvia, particularly on the height NW. of Leicester town, where an immense cap of alluvial clay, so abounds with hard chalk and these bolders of limestone, that at Burstal Cliff-house on the road to Thurnby, they have been dug for burning lime: the covering matter at Chellaston gypsum quarries S. of Derby, is of this same clay; but lime-bolders do not there so much abound, as they do near Leicester and thence in isolated patches towards Market Harborough, forming there the principal material for repairing the public road. The above with others are indications I think of vast tidal currents which have swept over all the surface

from SE. to NW., since or at the time, that the deposition of regular strata ceased, as observed p. 37.

It struck me, as among the curious circumstances of the basin of Paris, that the strata throughout it are horizontal, at least only one instance to the contrary is mentioned (p. 43), and that faults or such like derangements of the strata are nowhere mentioned.

All the southern parts of Derbyshire and part of Leicestershire adjoining, abound with red earth or gypsous marle (entirely without fossils I believe) that is in like manner horizontal throughout, with three exceptions only, that I could discern: viz. at the S. of Burton Trent bridge on the E. side of Catton in Croxall, and NW. of Stretton-le-fields; the second of these only has any considerable inclination: although tilts of the strata are so rare in our gypsous strata, yet many faults seem to occur in them: a most tremendous one forms the northern limits of the gypsous marle for 40 or 50 miles, and four or five isolated lime and coal districts within it, seem surrounded by great faults and to be intersected by numerous others, occasioning as inclining and dislocated strata at Breedon, Cloud's hill, Measham, &c. as can almost any where be found.

It is stated by our authors (p. 54 and 55), that their Vth and VIth soils are placed side by side of each other: if these be really different soils, and not the same somewhat modified in their substances, the fossils in one of them also not being preserved, (of which I entertain some suspicion) a fault must, in all probability, range between the two places where these soils have been noted, which elevates one of the soils or depresses the other. I would also here suggest as a query, whether their VIIIth soil be not a similar repetition of part of these same soils? Its fresh-water shells are not sufficient, I think, to prove it an upper soil; and in the latter part of the Memoir we are not presented with the same evidence of the superposition of the different soils as we are with respect to the first five, which may however have arisen from the haste in which the Memoir seems to have been prepared.

I do not remember to have seen any where, the appearance of limestone strata upon the chalk in England, or to have heard of any such from Mr. Smith, or otherwise: of gypsum I think we may safely say that there are no strata: but crystals of selenite abound in the London clay, and there are numerous patches of plastic clay, used in some places on our chalk hills for pottery and brick-making, which seem to answer pretty well to the description of

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some of that within the basin of Paris; but we cannot I think infer, that this same plastic clay underlays the London clay, in the neighbourhood of London, as there is generally a sand here immediately upon the chalk, but which varies greatly in thickness, from many yards (as in the sand-pits by the Thames between Greenwich and Woolwich) to a few feet only or perhaps inches, in the borings of some of the modern wells in and near the metropolis: still however this sand has always I believe been found in this district, and it has generally been forced up with great violence into the well, by the water issuing from the joints in the chalk beneath, on first pricking this spring with the auger, or on any after occasion when the column of water in the well is much and suddenly diminished, as often happens by the pumping at Meux's brewhouse and other places.

Mr. Smith and myself have been used to consider the London clays as regular strata, and the layers of small regular flattened chert-stones which it contains, as nodules peculiar thereto, and not as *rounded pebbles* and indicating an alluvial origin to these clays: I thought these conclusions well warranted by the regular beds of which this clay in many places consists; its regular layers of ludus helmonti; the uniformity in the shape of its chert pebbles, many of which appear concentric in their structure, and by their having an external coat or covering, which is nearly similar in all of them: but an able student under Mr. William Smith, of the same standing with myself, Mr. Benjamin Bevan of Leighton Buzard, Beds., whose situation as engineer to the Grand-Junction Canal Company, has for some years past given him better opportunities of examining the northern edge of the London clays than I have possessed, has for some time held the clays and sands above the chalk to be alluvia, and produces instances of chert pebbles taken from their bed on Rislip-common, Middlesex, in a pit where sand containing layers of these pebbles has chalk in its bottom, to prove these pebbles to have been rounded, and even some of them to have been broken and since partially rounded (which is perhaps the most unerring test of rounding, that we meet with). It has also been suggested by this gentleman, that the peculiar dark-coloured and uneven coat of these pebbles, is occasioned by a partial decomposition which they have suffered, since their rounding, the effects of which are also visible in the concentric stains within many of them, which give them so very nearly the appearance of original nodules. In candour I ought also to state, that the accounts which I have collected of the

sinkings

sinkings of various new wells, (on some of which I have been consulted) near the metropolis, have exhibited less of regularity in the succession and thicknesses of the strata, than I had been led to expect: also, that at Alford in Lincolnshire, where numerous very shallow wells are sunk and holes in them bored, through this clay and the sand under it into the chalk, from whence the water rises and perpetually overflows the surface, I found this clay and sand much thinner than it usually is about London. It is my wish, to suspend entirely my opinion on the above interesting questions, until many more facts are collected, and especially until the situations of our principal assemblages of fossil shells above the chalk at Hordel, Reading, Woolwich, in the shell marles of Suffolk, &c. &c. are ascertained, and they have been examined and minutely described by a competent conchologist, and such descriptions have been carefully compared with the drawings and details by our authors and M. Lamarck: an undertaking which I wish much to press on the immediate attention of the societies above mentioned.

If no part of the chalk be elevated more than 50 feet above the Seine (p. 43), it should seem probable, that the 50 chalk fossils mentioned (p. 42) belong to about only one-eighth of the whole thickness of the chalk strata, at its top.

In commencing a very extensive Mineralogical Survey, like Mr. Smith's manuscript Map of England, Wales, and part of Scotland, it will perhaps be best to follow his example, in selecting only such strata as usually form *distinct ranges of hills* through the country, by their bold or sudden endings, to form his classes or principal assemblages of strata, that are to be distinguished each by a different colour in his Map and Sections, without much regarding the mineral characters, or characteristic fossils of the several thin strata that compose each of them, leaving these to be enumerated and described in written details and in local sections on a large scale. In commencing more local, but yet considerable surveys, such as my square of map including Derbyshire, or the basin of Paris, the bold endings of particular strata ought I think still to have a principal share in determining the selection of such as are to have a different colour assigned to them; the other considerations should be the width or extent of surface which is made by the several strata, and the distinct mineral characters of particular thick strata or beds: a few thin beds or strata which happen to have very striking characters either in their appear-

ance,

ance, economic uses, or conspicuous fossils, may have a colour assigned them, especially if such fall between, and not so as to subdivide thick masses, selected for separate soils, or genera on the principles above.

In coal-districts or others, where grit or limestone alternate with argillaceous strata, it will be found right perhaps, to select all the thick masses of rock, which are sufficiently separated by clay-shists and other argillaceous strata, to have different colours assigned them, either using some general colour for the clays formed on the surface by all the argillaceous strata, or assigning a colour to each of these argillaceous assemblages, according to the number of the strata and thicknesses of them, so that the map be not over crowded. The transitions from light to heavy or dry to wet land, made by the above two classes of strata, are the best defined characters that we meet with in mineralogical surveying; and are indeed almost the only ones by which the farmers and occupiers of the lands can give any assistance by their information, except while pacing over every part of the surface with the surveyor, in answer to his questions, as they may arise.

The plan which M. Cuvier and M. Brogniart seem to have followed, in assigning colours to their soils (p. 41), and dividing some parts of the Paris strata into their ten formations or soils, seems to me much inferior to the above; and in particular their sea-sands, freestone without shells, and fresh-water soils, appear calculated to lead to no useful result, but rather to mislead. The more popular and obvious the divisions of the strata are made on mineralogical maps and sections, the sooner will the geological facts they convey be understood and rendered useful; for after all that can be said, they must, in the present state of our knowledge at least, be but arbitrary classes or divisions, exhibiting the order of super-position, principally.

The freedom of the remarks which I have ventured to make on the opinions and writings of geological observers of such high celebrity, will I trust in candour be ascribed to their true motives,—a desire to render justice to a valuable friend and to our country, and to advance and perfect the science, which of all others seems to me to claim our serious cultivation and attention. An apology may perhaps be necessary to your other correspondents and readers for the great number of your valuable pages which I have occupied.

I am, sir, Your obedient servant,

JOHN FAREY.

February 12th, 1810,
No. 12, Upper Crown-street, Westminster.

XX. *Report on a new Navigable Canal proposed to be cut from Okeham to Stamford, and from thence to the Town of Boston.* *By* THOMAS TELFORD, *Esq.*

THE gentlemen of Lincoln and Rutland having for some time been turning their attention to the discovery of the best means for extending and improving the benefits already resulting from inland navigation, in such a manner that the greatest local advantages to the town of Stamford may be united with the general increase of the agricultural and commercial prosperity of a large district of surrounding country, and having employed Mr. Telford to take a view of the different lines of country embraced by their plan, have received the following

REPORT.

"Having, in compliance with the directions of the committee, carefully examined the districts of country, extending from Stamford westwardly to Okeham, and eastwardly to Peterborough, Spalding, the Foss-Dyke, and Boston;—I shall state my ideas with regard to the sundry lines of inland navigation, which appear best calculated to promote the most perfect intercourse, and, consequently, the general prosperity of the country.

"The whole of Leicestershire being already intersected from north to south by an inland navigation, and also from west to east by a line of canal, passing by Melton Mowbray to Okeham in Rutlandshire; there now only remains to be considered and determined, the most adviseable mode of proceeding from the last point, eastwardly, to the ports situated upon the great bay or inlet, called the Wash; and thereby opening a direct and commodious communication with a point on the east coast, nearly centrical between the Trent and the Thames.

"The town of Okeham, at which the last-mentioned canal terminates, being situated upon the summit of the ridge of land which occupies this part of the country, and from which the adjacent streams have their course to the river Welland, affords an opportunity of choosing a line of canal, either down the river Wash, or Guash, which falls into the river Welland, about a mile and a half below Stamford bridge; or down the river Chater, which unites with the Welland about two miles and a half above Stamford.

"The river Wash, occupying the valley nearest to the

An arranged Table or Section of the STRATA *in the Environs of* PARIS, *in the relative Positions in which they have been described by* M. Cuvier *and* M. Brogniart ; *referring to the Pages of the Translation of their Memoir, in the Philosophical Magazine, vol.* 35, *p.* 36 *to* 58. *By* Mr. John Farey.

X.	Sand (probably alluvial), on the Heights of Beauce, &c., west and south of the basin of Paris. p. 38, the particular description of which is omitted, though promised at page 40.	
IX Art.	Alluvium, in putrid marshy vales, containing skeletons of large animals, trees, &c., of various kinds. p. 58.	
VIII Art.	Siliceous limestone, which decomposes and contains *Burrstones*. It contains 4 kinds of *fresh-water* shells and gyrogonites ; also siliceous wood and vegetables at Longjumeau ? p. 56 and 57.	
VII Art.	Freestone without shells. p. 56. Loose sand used in the Arts. Freestone without shells. p. 56.	
V Art.	Siliceous Freestone and *Sea sand*, with numerous shells (such as are found in the coarse limestone of the IIId formation). p. 54. Argillaceous strata without organized fossils. p. 54.	VI. Freestone without shells. } p. 56. Argillaceous marles. } p. 56. Siliceous limestone, containing *burrstones* but no fossils. p. 55. Plastic clays. p. 55.
IV Art. Marine Formation.	Argillaceous sand, without shells. p. 52. Marle, with several seams of brown small thin oyster-shells. p. 52. Whitish marle without shells. p 52. Marle, with a seam of very thick large oyster-shells. p. 52. Marle beds, containing sea bivalve shells. p. 52. Yellow argillaceous marle, with fragments of 5 different sorts of sea shells, and bones of a fish. p. 52. Marle, in 4 or 5 beds, without. p. 52. Green potters' earth, thick, with argillo-calcareous nodules, and small pieces of sulphated strontian, but no fossils. p. 52. Yellow scaly marle 2 feet thick, with small pieces of earthy sulphated strontian, and a thin seam of small elongated telline shells. p. 52.	
IV Art. Fresh-water Formation.	Argillaceous and calcareous marles, thick, without fossils. p. 52. Marle beds, calcareous and argillaceous. p. 51. White calcareous bed, with large siliceous trunks of palm-trees ; and with fresh-water lymnea and planorbis shells at Romainville. p. 51. 1st Mass. Gypsum in thin beds, and five marle-beds. p. 51. Marle strata. Columnar Gypsum, the coarse prisms have several [planes. p. 50. Marle strata. Siliceous Gypsum, intimately blended. p. 50. (These contain skeletons of unknown birds and quadrupeds ; bones of tortoises, skeletons of fish, and of mammiferæ ; and sometimes fresh-water shells. p. 51.) Marle strata. 2d Mass. Gypsum beds, with thin marle beds, and small pieces of sulphated strontian, compact marbled gray scouring-stone, and fossil fish. p. 50. Marle strata. 3d Mass. Selenitous Gypsum, in thin beds with numerous marle beds, some of solid calcareous marle that contains coarse crystals of lenticular yellow gypsum, and others of very scaly argillaceous marles containing menilite silex, but no organized fossils. p. 50.	
III Art.	Hard calcareous rubbly marle with dendritic joints, and soft calcareous marle beds, but no fossils. p. 48. Calcareous Sand, sometimes agglutinated, and containing horny silex (chert) at Neuilly quarry, quartz crystals, and variegated crystals of carbonate of lime. p. 48. Hard calcareous rubbly marle with dendritic joints, and soft calcareous marle beds, but no fossils. p. 48. 3. Hard earth, containing seams full of small long white striated telline shells. p. 48. 2. *Buildingstone-rock*, sometimes soo soft. p. 48. Gray and yellowish strata, sometimes very hard, sometimes soft. (These contain camprey and roundish Venus shells, with prodigious quantities of tuberculated cerites. p. 48.) 1. Soft greenish earth (the green bank), exhibiting on its lower surface brown marks of leaves, and stalks of vegetables. p. 48. Coarse Limestone in beds, alternating with thin marles and clays : the lower beds are sandy, contain greenish earth, and though hard, decompose quickly on exposure. They contain extraneous fossils in good preservation, of the following genera, viz. calyptreæ, camerines, cerites, fungites, madrepores, muscles, nummulites, oysters, periwinkles, pinnæ, porpytes, pyrulæ, solens, large tellines, terebellæ, &c., amounting together, to more than 600 species ! Many shells similar to these, occur also in the Vth formation. p. 46, 47, 48, and 54. Sand, coarse, red or blueish gray, without fossils. p. 45 and 46.	
II Art.	Plastic Clay ; white, gray, slate-gray, reddish-gray, and red potters' clay, from 4 inches to 52 feet or more in thickness, contains no fossils, except, perhaps, fragments of bituminous wood, at Rolleboise. p. 44 and 45.	
I Art.	Chalk, with many layers and nodules of flint. p. 41. Chalk in distinct beds, and with few flints, at Bougival. p. 41.	In these are found, ananchites, anomites, belemnites, caryophyllæa, a cranium, fibrous shells, lituolites, millepora, muscles, oysters, pinnæ, polypiers, porpytes, shark's-teeth, spirorbis, vermiculites, &c. comprising in the [illegible]

the town of Okeham, has induced Mr. Whitworth, in his survey, to follow that stream to its junction with the Welland; but by that line, though sufficiently regular in its descent, being obliged to skirt the north side of the high ridge of land which lies to the north of Stamford, an awkward circuity is created before it can reach the town, so that the distance by the navigation between Stamford and Okeham would be nearly nineteen miles.

"In proceeding from the sea-coast to the interior of the country, the vessels navigating that line must either pass at the distance of about one mile and a half from Stamford; or having come up to the town, must return the same distance, along the same line, to get into the valley of the Wash.

"These circumstances, in my opinion, render that line objectionable, and inferior to another line which may be obtained by means which Mr. Whitworth has himself partly pointed out. This is by continuing the head level from Okeham over the south field, along the before-mentioned line, about a mile and a quarter from Okeham; and from thence, instead of locking down the Wash valley, to continue through Egleton into Gunthorpe, and there locking down to reduce the embankment across the Wash valley, so that the cutting through the ridge, at Martinsthorpe, shall afford earth sufficient for its construction. This will enable the line to be carried into the Chater valley, without being encumbered with a tunnel, which Mr. Whitworth, from a cursory view, apprehended necessary. The valley between Gunthorpe and Martinsthorpe will also be a convenient place to receive the feeder from the reservoir proposed to be formed at Braunston.

"This line, having crossed the ridge at Martinsthorpe, should be locked down nearly to the bottom of the valley of the Chater, and be carried down the north side to near Ketton, where, in order to avoid the village, it should cross to the south side of the valley. After passing the village, it must be again brought to the north side, and be continued to the most favourable point for crossing the river Welland, below where the Chater has fallen into it. After crossing the Welland, the line should be carried along the skirts of the wood, as nearly as possible, in the division between the uplands and the meadows, and it should fall into the river above Stamford bridge, in the most convenient way, to enable wharfs to be formed on each side of the river. For accomplishing this object the opportunities are ample, without interfering with buildings. I prefer the south

south bank of the river Welland, from near the junction of the Chater, because it is more favourable ground for a canal than the north bank, which is composed of loose rock; and because, if the proposed line to Harborough be executed, the last-mentioned two miles and a half would answer the purposes of both navigations. The length of the line between Stamford and Okeham, as nearly as I can at present make out, would not exceed fifteen miles, being shorter than the line laid down by Mr. Whitworth by four miles. Besides that this line arrives at Stamford from Okeham by a shorter distance than the other, it will be more satisfactory to the principal landowners in the county of Rutland; it bears more equally upon the general population of that county than the other does: and it will for ever fix, more directly, the intercourse by inland navigation through the town of Stamford.

"Proceeding from Stamford towards the sea coast, it will be necessary to continue upon and improve the present navigation of the Welland to the second lock, being a distance of about three miles; but from thence to the sea, it will cost more to render the old navigation perfect, and acquire a proper outlet to the sea, than will construct an entirely new canal navigation to the town of Boston.

"I am therefore of opinion, that, at or near the second lock, a line of canal should depart from the north bank of the Welland, and,—passing along a line between the uplands and the meadows to the westward of Tallington, and immediately eastward of Barholm, to the west of Kate's Bridge,—it should enter the Car Dyke, along which it should be carried, until that ancient work approaches the South forty-foot drain; and opposite Billingborough, or Horbling, and then should proceed along that excellent drain, to the town of Boston.

"From the Welland to Car Dyke, an entirely new canal must be formed. Its passing, as much as practicable, between the uplands and meadows, will be favourable to the adjoining properties, and afford good ground for the necessary works. Along the Car Dyke, in many places, it has been preserved as a drain, and will become a part of the proposed navigation. It will then also form a more perfect boundary to the fields which have always been separated by it, and when they are accommodated by necessary bridges, the adjacent properties will be much improved. Along the South forty-foot drain, with the exception of deepening its bottom from half a yard to two feet, and widening

widening the lock at Boston, to suit the breadth and draft of vessels which navigate the Leicestershire canals, very little more is required to render it an excellent navigation.

"By the canal line being carried along the Car Dyke, it will not at all interfere with the navigations of the rivers Glen and Bourne, because it passes above the places where those navigations terminate: and as the waters of those rivers will be passed under the canal, no apprehensions can be entertained that their usual supplies will be lessened.

"The river Welland cannot be injured, because, during winter and rainy seasons, the superabundance of water will be more than sufficient for all purposes: and, in dry seasons, as the supplies of water for this navigation are proposed to be drawn from reservoirs situated near the summits of the country, they will be passed through locks in the upper country, which are at least double the depth of those in the lower country, so that unless there is more than double the quantity of business in the Fens, to what is carried through the upper country, no additional water can be required. Besides, the leakage from the upper locks will be much more than from the lower ones, consequently the surplus water must fall into the Welland.

"The South forty-foot drain proprietors will have no apprehensions of too great increase of water, in their drain, when they consider the interest the canal proprietors will have to preserve their water, by constructing very shallow and perfect locks, and by adding to this, the great extent of surface each lockfull has to spread over, and the regulation which will constantly be taking place by the lock at Boston.

"The conservators of the port and haven of Boston, besides the certain prospect they will have of increasing the prosperity of the place, must be sensible of the evident advantage of deriving additional supplies of water to assist in scouring out and maintaining the bed of the river between Boston and the sea.

"The communication between Stamford and Boston, by passing along the line of division between the Upland and Fen countries, where the most populous market towns and villages are situated, will (exclusively of the thorough trade) be of great advantage to all the district of country through which it passes, and afford adequate tonnage dues in return; so that, upon the whole, this appears to be an improvement which will be generally beneficial, and will interfere, as little as possible, with any established rights.

"In order to render the inland navigation of this district

strict of country, and the connections with the interior districts more perfect, and to afford a fair competition of local advantages, I am of opinion that a canal should be carried between the Welland and the Nene: and the country appears to be particularly favourable for this junction. This line should depart from the Welland precisely where the branch to Boston does, and in a manner similar thereto. It should be carried in a line dividing the upland from the flat country, and terminating at or near Peterborough. By these means, the elevation would be small; and proper ground would be obtained for the canal works: and the canal, being supplied with waters, which now pass partly into the Welland and partly into the Nene, those waters would be turned by lockage to the respective rivers, so as to injure neither. The country through which the canal would pass is very populous, and requires communication: and the towns, and whole population of the valleys of the Welland and the Nene, would thereby have an opportunity afforded them to benefit by the navigation of all the rivers which fall into the great bay—with the choice of such of them as should best suit their interest and conveniency.

"Having, I trust, stated satisfactory reasons why the former surveyed line of canal should be abandoned, and having hitherto been enabled to recommend other lines only from a general inspection of the country, the committee will readily conceive, that, until regular and careful surveys and sections have been made of the new lines, it is impossible for me to enter into a more minute detail, either with regard to the precise situation of the lines, or the nature of the works required, or to form any correct estimate of the expense. But if it be judged adviseable, after this general explanation, to authorize me to proceed in getting these surveys and sections made, no time shall be lost in performing the service, and furnishing the committee with all necessary data for making an application to parliament.

"In the mean time, I may venture to state, that although from the quantity of lockage necessary to ascend to the canals, already made upon the summit of the country, the expense of the line from Stamford to Okeham will be fully equal to the general average of canals of similar dimensions; yet those from Stamford towards Boston and Peterborough, from their small elevations—the favourable nature of the ground—and from having, in the South forty-foot drain, about fifteen miles of canal nearly complete, as well as great facilities in the Car Dyke, will be greatly

greatly under the general average of expense; so that, upon the whole scheme, (embracing an inland navigation of from 60 to 70 miles,) taking into view its extensive connexions, there appears a fair prospect of ample remuneration for the adventurers.

THOMAS TELFORD.

Stamford, 8th Jan. 1810.

In consequence of the foregoing Report, it was resolved, at a meeting of the committee held at Stamford on the 8th of January, 1810,—That it appears to this committee to be impracticable to go to parliament in the now ensuing session for the sanction of the legislature to the plan which Mr. Telford has proposed, inasmuch as the necessary surveys cannot be in due time prepared, nor the notices given, nor the plans delivered, which are required preparatory to the introduction of navigation bills into parliament: but that Mr. Telford be directed to take the steps proposed in his report, and that every other proper measure be pursued for the introduction of the bill into parliament in the session following.

XXI. *On Crystallography. By M.* HAUY. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from vol. xxxiv. p. 466.]

If these ridges were subject to a different law, which gave rise, for example, to subtractions of two ranges, the sign would become $\overset{1}{D}\overset{2}{e}\overset{2\cdot 0}{E}P\underset{1}{B}\underset{2}{b}$. According to this it has been considered as settled, that the decrements represented by a large letter accompanied by any cypher would not implicitly contain similar decrements represented by the small letter of the same kind, or vice versa, *i. e.* for example, that $\underset{2}{B}$ would not implicitly contain $\underset{2}{b}$, or, vice versa, that when the second letter would not enter into the expression of the sign with a different cypher, we should not use the same cypher accompanied by a zero. In the first case each of the two letters expresses a decrement which is peculiar to the ridge or to the angle which it indicates; in the second, that which is affected by a zero shows that the angle or the edge to which it exclusively relates undergoes no decrement. Thus in the sign $\overset{1}{D}\overset{2}{e}\overset{2\cdot 0}{E}P\underset{1}{B}\underset{2}{b}$, B expresses a decrement by one range, which only takes place on the ridges contiguous to the upper summit A (fig. 73); $\underset{2}{b}$ indicates a decrement by two

two ranges, which only acts in the same way on the ridges contiguous to the lower summit. Finally, the quantities $\overset{2}{e}$ and $\overset{0}{E}$ ought to be thus considered independently of each other; the first as expressing a decrement by two ranges on the angles *e* solely, and the second as indicating zero of decrement on the angles E opposite to the foregoing.

I have enlarged on the detail of the principles of the method, in order to leave nothing to be desired, if it were possible, of what can be of use in enabling my readers to have a clear idea of the art, and put an observer in possession of the method of instantly representing a secondary crystal of a given form. But if any person confined himself to the simple comprehension of the signs employed in the system, and was only anxious to read without being able to write them, he would only require some simple and easily understood rules, which we shall here succinctly explain:—they will form a kind of recapitulation of the preceding details.

1. Every vowel employed in the sign of a crystal designates the solid angle marked with the same vowel on the figure which represents the nucleus; and every consonant indicates the ridge which bears this same consonant, or the face the middle of which it occupies.

2. Every vowel or every consonant is accompanied by a cypher, the value as well as the position of which indicates the law of decrement which the corresponding angle or edge undergoes. We must except the three consonants P, M, T, each of which, when it forms part of the sign of a crystal, indicates that this crystal has faces parallel to that which bears this same letter.

3. Every letter comprehended in the sign of a crystal is marked below with the cypher that accompanies it, on all the angles or all the edges which perform the same function with that which on the figure is marked immediately with the letter in question.

4. Every number added to a letter indicates a decrement, the angle or edge of which marked with this letter is the term of departure. If the number be entire, it indicates how many rows are subtracted in breadth, with the condition that every lamina has only the thickness of one molecule; if the number be fractionary, the numerator makes known how many rows are subtracted in breadth, and the denominator how many are subtracted in height.

5. According as the number is placed below or above the letter which it accompanies, it indicates that the decrement

ment descends* or ascends, setting out from the angle, or from the edge marked with this letter. If it be placed towards the top and to the right or left of the letter, it designates a decrement which takes place in the lateral direction to the right or to the left of the angle which bears the same letter.

6. When a letter is found written twice successively with the same cypher placed on two different sides, such as $^2G\ G^2$, or $G^2\ ^2G$, $^2A\ A^2$, or $A^2\ ^2A$, the two edges or the two angles which it designates, ought to be considered on the figure according to the same relative positions, *i. e.*, for example, as in the sign $^2G\ G^2$, the quantity ^2G indicates the effect of the decrement on the edge G situated on the left, and the quantity G^2 the effect of the decrement on the edge situated on the right.

7. When a letter bears the same cypher repeated both on the right and left, as $^3G^3$, it is applied indifferently to any one of the ridges G which it designates. It is the same with the letters which belong to the angles.

8. The parenthesis in such as $(\overset{3}{O}\ D^1\ F^2)$ designates an intermediary decrement. The letter $\overset{3}{O}$ expresses in the first place, that the decrement takes place by three rows on the angle O, and that its effect is ascending. $D^1\ F^2$ make known that for one ridge of molecule subtracted along the side marked D, there are two ridges subtracted along the side marked F.

9. Every small letter comprehended in the sign of a crystal, indicates the angle or edge diametrically opposite to that which bears the large letter of the same kind, or the figure in which the small letter in question is omitted as superfluous. We must except the letter *e*, which is always employed on the figure of the rhomboid, and which indicates, according to the principle, the angle opposite to that which bears the letter E.

10. When a sign contains two letters of the same kind, the one large and the other small, with different cyphers, the two opposite edges or the two opposite angles to which these letters answer, are considered as each undergoing exclusively the law of decrement indicated by the cypher added to the letter.

* We only allude here to the general progress of decrements, to which the particular cases refer that seem to form an exception. For instance, if the decrement be produced by one row on the angle at the summit of a rhomboid, then the face produced will be horizontal. But this decrement comes within the description of those that are descending, and of which it is as it were the limit.

11. Every letter, whether large or small, marked with a cypher which has a zero after it, shows that the decrement indicated by this cypher is null on the particular angle or edge to which this letter refers.

We have omitted the applications which would be necessary for understanding these rules, if they had been presented on our first setting out. These applications are found already in the detailed explanation, which we have previously given, of the principles of the system, and the perusal of which is presumed to have preceded that of these same rules.

OF INDETERMINABLE CRYSTALLIZATION.

When the crystalline molecules disseminated in a liquid experience obstacles which affect their tendency to reunite in conformity to the laws of their mutual affinity, the forms which result from their aggregation have no longer that regularity which belongs to an exact and precise determination. Their ridges are obliterated, their faces are curved, their pyramids are sharpened. Hence the crystals called *lenticular*, or which imitate the form of a lentil; *cylindroids*, the prism of which is rounded off; *acicular*, or similar to needles; &c.

If a multitude of small indeterminable crystals are so intimately connected with each other that they form only one body, we then consider this body as a particular being, and hence the substances which we call *striated*, *fibrous*, &c., and which are formed by the junction of an infinite number of crystalline needles, sometimes parallel, sometimes divergent, and at other times crossing in different directions.

Lastly, The appellation *amorphous* has been given to substances which present, as it were, the last degree of confused crystallization, and the vague and indefinable form of which is, as it were, *mute* to the eye of the observer.

OF CONCRETIONS.—The formation of the bodies which we have hitherto mentioned, particularly of crystals properly so called, essentially depends on two conditions only: one of which is, that the molecules of these bodies should be in the state of integrant molecules; and the other, that they should be kept in suspension in a liquid capable of abandoning them to the attraction which solicits them towards each other. In short, every thing is regarded as passing in the same manner as if, the force of gravity being null, the liquid was not coerced by the sides of any surrounding matter, and as if the crystal itself remained isolated

lated in the liquid, without having occasion to be supported.

This is not the case with the bodies which we are now about to consider. The modifications which they present are owing to certain local circumstances, such as points of attachment, props or moulds which influence their form. We unite all these modifications under the common denomination of *concretions,* which in the ordinary acceptation signifies a *congealed or fixed substance.*

But in order to fix in a more precise manner our ideas on this head, we shall comprehend under the term concretions, the different bodies, the aspect of which depends, partly at least, on their molecules being in contact with other bodies. We shall now give an idea of the various circumstances which contribute to vary this appearance.

1. *Stalactites.* The water which filters into the fissures of stones situated in the arched part of subterranean cavities, or which oozes through the lax and porous texture of these vaults, arrives at the surface after hollowing out certain stony molecules which are united to it in any way. The drops which remain suspended from the arch during a certain time, undergo a desiccation, which commences on the external surface; and the stony molecules which the liquid gets rid of, exerting their attraction on each other, and attracted at the same time by the side of the vault which they adjoin, form in this place an initial tube, or kind of small ring. This rudiment of tube increases and grows longer by the intermedium of other drops, which succeed to the first, conducting new molecules which the orifice of the tube attracts in its turn. Sometimes this tube preserves the form of a hollow cylinder, similar to a quill. But frequently it increases in size, and is enveloped with concentric layers, the matter of which is furnished by the liquid which descends along the external surface. It then becomes a thick cylinder or cone; and sometimes the molecules hollowed out by the drops which thus flow into the interior of its canal, finish by obstructing it entirely. These different modifications are peculiarly sensible in bodies which belong to carbonated lime.

But a part of the liquid, on falling from the arch upon the ground, forms there other depositations composed of strata generally undulated, or protuberances, the figures of which vary ad infinitum. Lastly, the liquid which flows along the lateral partitions gives rise to bodies, the form of which we might compare to that of a drop of congealed water.

Stalactites are those bodies which are formed in the arch of the vault; and *stalagmites* are those which originate from the falling of the liquid on the ground. It is, however, much more convenient to call both *stalactites*, as it is sometimes difficult to distinguish between the two kinds of formation, when the bodies under consideration have been removed from their original position.

2. *Incrustations.* In the preceding concretions, the aggregation of the molecules depends more especially on the evaporation of the liquid which has flowed over them. Other concretions, which have been called *incrustations*, *tufs*, and *sinters*, proceed from a kind of precipitation of the molecules originally suspended in the liquid. The latter are sometimes deposited on the surface of different organized bodies, particularly on those which belong to the vegetable kingdom, and sometimes cover the inside of certain bodies, such as sewers or drains.

When the liquid is introduced into a subterranean cavity of small dimensions, where it can remain, the stony molecules incrust the sides of this cavity, which is generally of a round form, and sometimes end by studding it with crystals. This is what has been called *geode*. Some of these bodies contain a solid and moveable nucleus, or a pulverulent earthy matter*: of this description also are certain pieces of silex found in marle. Sometimes also the geode is entirely filled with a matter which may be distinguished by the naked eye from that of which it is itself composed.

It may also happen that a substance may be incrusted with crystals of a different nature, by being as if moulded along with them. For instance, we are acquainted with crystals of metastatic carbonated lime incrusted with quartz, and sometimes the quartzous envelope remains empty after being separated from the crystals which it concealed.

3. *Pseudomophoses.* There exists a third order of concretions which we call pseudomorphoses, *i. e.*, *bodies which have a false and deceitful figure*; because the substances which belong to this order present in a very remarkable manner foreign or strange forms, which they have in some measure obtained from other bodies which had received them from nature.

When the type of this apparent transformation is a shell, it happens frequently enough that the shell still covers in whole or in part the substance, which is as if moulded

* It is probably from this that the term *geode* is derived, i. e., a body which contains earth.

into

into its interior *, and then nothing appears simpler than the explanation of the fact, by the introduction of a liquid charged with stony molecules into the cavity of the shell; and this observation leads to a similar explanation of the formation of the kinds of nuclei modelled into shells, which we meet with isolated and stripped of every envelope.

Sometimes the shell itself has been penetrated by another matter generally siliceous, which has been substituted for the cartilaginous substance of which this shell had been partly composed †; and it may happen in this very case that the interior of the shell has remained empty. It is no longer, properly speaking, a pseudomorphosis. It is a fossil which has merely become more stony than it was before.

This last kind of modification takes place equally with respect to the bones and to the other solid parts of animals which are found immured in the bowels of the earth; *i. e.*, they may pass to an almost entirely stony state, by the help of a substance which supplies the place of their cartilaginous part.

The case cannot be the same with vegetable productions as with shells. They have no testudo, or envelope, which can exist after the destruction of the interior substance, and serve as a mould to a stony or other substance for receiving an impression of their form. If we supposed that one of these productions, such as a portion of the branch of a tree, were entirely destroyed, so that the cavity which it occupied in the bowels of the earth remained empty, we could conceive that a stony matter might afterwards fill this cavity and there be modelled to it. In this case the new body would resemble externally the branch of a tree; it would have the appearance of knots and wrinkles, but its inside would not present any trace of organization, and it would only be, as it were, the statue of the vegetable production, which it would have displaced.

What is generally called *petrified wood* is a much more faithful imitation of real wood. On a transverse section we distinguish the appearance of concentric layers, which in the living tree must have proceeded from its increasing in thickness; all the principal lineaments of organization are preserved to such a degree, that they sometimes serve to enable us to recognize the species to which the tree belonged which has undergone petrifaction.

* De l'Isle Crystall. tome ii. p. 161.

† We know that shells, as well as the bones of animals, are formed of two substances; the one calcareous, which is not susceptible of putrefaction; the other cartilaginous, membranous, or fleshy, which may be destroyed by fermentation.

Among the different explanations which have been given of this phænomenon, that which seems to be most generally admitted, although not exempt from objections, consists in supposing that the stony matter is substituted for the vegetable in proportion as the latter is decomposed; and because the substitution takes place successively, and as it were molecule by molecule, the stony particles, in arranging themselves in the places rendered empty by the disappearance of the ligneous particles, and by moulding themselves into the same cavities, take the impression of the vegetable organization, and copy the traits of it precisely.

The mineral kingdom also has its pseudomorphoses. We find some substances of this kingdom under crystalline forms, which are only borrowed; and it is probable that, in some cases at least, the new substance has been substituted gradually for that which has ceded its place to it, as we suppose takes place with respect to petrified wood.

The various pseudomorphic bodies imprint their form on the matter which surrounds them, and frequently also the impression serves as a cell for an organic substance which is simply in a fossil state, or which has received a certain degree of alteration only. This takes place in particular with respect to the ferns and other plants of the same family, the form of which is moulded on a schistous matter, as we shall afterwards more fully detail.

We generally denominate *petrifactions* all the variously modified substances which we have mentioned, even those which only present impressions of animal or vegetable productions. Daubenton applies this term only to bodies which, in their natural state, being partly stony and partly cartilaginous, such as shells, have become entirely stony.

As we merely purpose to mention a few examples of the modifications in question, and not to unite them methodically under one and the same point of view as several authors have done, we shall confine ourselves to the enunciation of some of them in speaking of the substances which have formed their secondary matter, and shall adopt the nomenclature to this method of classifying.

We ought not to omit that there are also pseudomorphoses, which arise from the substitution of a metal in the room of an organic body. Sulphurated iron presents several examples of this kind of metallization.

By referring to all that has preceded, we may define in the following manner the different concretions of which we have given the description:—

The

The *stalactite* is a concretion composed of successive layers of a circular or undulated form, which is the effect of desiccation.

The *incrustation* is a concretion in the form of a crust applied to the surface or to the interior of a body. To this we may refer the *geode*, which is a concretion in the form of an envelope, spherical or nearly so, sometimes empty and sometimes containing a nucleus.

The *pseudomorphosis* is a concretion endowed with a form foreign to its substance, and for which it is indebted to its molecules filling a space formerly occupied by a body of the same form.

[To be continued.]

XXII. *On a Hard Artificial Stone that generates a considerable Quantity of Heat during its Consolidation; with the Application of this Fact to the Cause of Volcanic Fires**.

The artificial stone about to be described presents a remarkable example of the great degree of solidity which water, in certain combinations, can acquire.

Water forms more than half the weight in the composition of these stones: the other ingredients are one part of sulphuric acid, and two parts of burnt clay (bricks or earthen-ware) reduced to powder.

The mixture of these substances yields, in fact, a solution of sulphate of alumine: but when in the mixture circumstances are favourable to their reciprocal action, heat is speedily produced, and the quantity evolved is sometimes so considerable as to produce ignition in the mass.

If from 25 to 30 hundred weight of materials be employed, this extraordinary phænomenon lasts for more than an hour.

The most remarkable circumstance is, that if no water be added to the mixture, when the reaction of the substances upon each other is the strongest, the mass, although still liquid, suddenly acquires a great degree of solidity: the heat which it produces is augmented, and the substance afterwards becomes almost entirely insoluble.

This last property being acquired by a mixture calculated to yield very soluble salts, proves that there is a great penetration of the earth by the water and acid, as the whole mass forms a stony composition only.

* From M. de la Metherie's *Journal de Physique.*

The stones here alluded to, although apparently possessing all the properties just described, are not insoluble: they were prevented from acquiring that state, as they would then be useless. But as this composition, with the exception of its insolubility, has all the external characters of the hardest stones, it possesses some claim to attention. Might it not, after having been softened by a heat superior to that of boiling water, be employed with much advantage as a cement, or to cast into models of statues or vases, &c.? Bodies formed of this artificial stone must be protected, however, from the influence of water or moisture.

What also contributes to the interest excited by this composition is, that its analogy with the stones of solfaterra, and the peculiar theory of its formation, will not require us to recur to the hypothesis of subterranean fires, to account for volcanic eruptions.

In short, as water, by merely passing in an instant from the liquid to a solid state, develops such a considerable degree of heat, may it not be the immediate cause of volcanic eruptions? May it not also be to the slow and progressive passage of water to the solid state, that the heat found at great depths in the interior parts of the globe is owing? Lastly, May not the heat developed in the animal and vegetable organs be also owing to water?

The above suggestions are thrown out with a view to call the attention of chemists, mineralogists, natural philosophers, and physiologists, to a subject which cannot fail to derive much light from their concurrent observations.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

This society on the 1st and 8th of February was occupied with reading a letter from Mr. Gibson of Manchester to Mr. Thomas, describing an extraordinary fœtus, having two heads, only one body, two arms, two legs, and of both sexes. There was nothing in the physical organization of this monster which could apparently have impeded the vital functions; its two heads were perfectly formed and joined side by side, that on the right being masculine and that on the left feminine; both were united to one spine; and although it had two hearts, only one stomach was

was found. The nerves from the head on the right side passed to the right arm and leg; those on the left to the left arm and leg; so that, had the creature lived, one head and mind would have directed the right side, and the other the left. The organs of the two sexes were very distinct, and the uterus was found in the bladder of the male. The author described the physical structure of this *lusus naturæ* very minutely, but the details would not be interesting.

Feb. 15. A paper on uric acid, by Mr. Brandè, communicated by the Society for improving Animal Chemistry, was read. The author related the effects of the alkalies and lime on the uric acid and phosphats, in patients labouring under the influence of calculi, but in none of the four cases which he stated were they successful in giving relief or curing the disease. Magnesia, however, had the desired effect, and brought off in the urine great quantities of uric acid and phosphats, in the form of triple salts. The discharge of these salts, after taking small doses of magnesia, was so copious, that the patients were radically or effectually cured in two or three weeks. The suggestion to use magnesia was made by Mr. Hatchet, who knew of nothing so capable of acting on uric acid, and experience has confirmed his conclusion.

Feb. 22. In consequence of the indisposition of Sir J. Banks, Mr. Marsden was in the chair, when the reading of a supplementary paper, by Dr. Herschel, on coloured concentric rings, commenced. The present object of the author is to explain and elucidate the positions laid down in his former papers on this subject, and in some measure to insure to himself more completely the sole merit of discovering the red bow, as Newton did the blue one. The introductory remarks chiefly referred to the 42d and 43d sections of the author's preceding paper, in which the nature of the red rings, the transmission of light, and the prismatic colours, were particularly discussed. The conclusion of this paper was postponed till next meeting.

SOCIETY OF ANTIQUARIES.

Mr. J. A. Repton presented to the society a series of designs of wooden houses, windows, or other parts of buildings constructed of wood, in order to show the origin and progress of architecture in wood throughout England. His views included the principal structures of timber in London, Essex, Suffolk, Norfolk, and Lincolnshire. In the explanatory observations which were read to illustrate the views, it was stated that sash-windows were not introduced into this

this country till the age of Charles I., and that they did not become general before the beginning of the last century. The sashes were originally constructed of very thick timber, and the joinings were left in square pieces in order to add to their strength, as it was then believed.

As an appendage to these views, the design of Melkhouse-street, a curious old rustic building, entirely of timber, near Ashford, Kent, was exhibited to the society. The drawing was executed by the late F. Grose, and presented considerable variety in its architectural ornaments. The design was made in 1760, and has not yet been engraved.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this society, on Saturday the 13th of January, the Rev. Dr. Macknight read a mineralogical account of Ben Ledi, and the environs of Loch Ketterin. The description of the rocks in that district (which consist of mica-slate and clay-slate, with an overlying conglomerate, formed at a lower level from the débris of primitive mountains) tended, in the author's opinion, to illustrate one branch of the Wernerian doctrine respecting the order of formations in the mineral kingdom. It also appeared, in confirmation of another principle in the Geognosy, that the direction from SW. to NE. of the strata composing the Highland mountains, corresponds to what has been observed in general relative to the bearings of the primitive strata in the crust of the earth. Such an uniformity of direction, it would seem, could have resulted only from the action of powers in nature that are slow and regular in their operation; and must be referred to some original law, which later discoveries render it probable may be found to depend on the constitution of the terraqueous globe in regard to magnetism and electricity.

At the same meeting the secretary laid before the society a communication from Mr. William Scoresby junior, of Whitby, comprising a meteorological journal of three Greenland voyages, with remarks on the effects of the weather on the barometer in Greenland, and on the different crystallizations of snow to be observed in high latitudes.

XXIV. *Intelligence and Miscellaneous Articles.*

ON PROCURING AN EQUAL TEMPERATURE.

To Mr. Tilloch.

Sir, On accidentally looking over a volume of the Medical Journal for 1801, I found a short but pleasing account of the effects of the climate of Madeira in cases of pulmonary tubercles. The communication is dated in January in the above year, and is addressed by Dr. Adams, then resident in that island, to a medical friend in this country. It concludes with the following suggestions:—"These are, I believe, the principal inquiries you wished to make:—it is true they are of little consequence compared to the important fact you have in view. It is however satisfactory to trace probable causes, *and it may be well worth your while to try whether spacious buildings, regularly heated, safely ventilated, and large enough to admit of necessary exercise, may not answer the purpose for such whose want of means, of courage, or of leisure, prevents their taking a voyage to a more genial climate.*"

It is not my wish, by sending you the above, to detract from the claims of Dr. Pearson, with whom the same idea seems to have originated: perhaps that truly respectable practitioner is not even aware of the existence of the passage in question. It is but fair, however, that the claims of others should be recognized, when the public, as in the present instance, begin to reap the benefits of their suggestions.

I am, &c.

X. Y.

MAGNETISM.

Mr. Leopold Vacca has discovered a method of communicating magnetism to a bar of iron without a magnet.

He takes a bar of iron of about three feet in length, which gives no sign of possessing any magnetic virtue as long as it lies in a horizontal position: but it possesses this in a very sensible degree when placed perpendicularly. These signs disappear again when it is laid down horizontally, and appear again when it is lifted up vertically.

A small bar of steel rubbed several times in the same direction, against the extremity of the other bar, when situated vertically, acquires magnetism: whence the discoverer concludes, that magnetism may be communicated to a body, without either a natural or an artificial magnet.

COBALT.

COBALT.

Those interested in the prosperity of our porcelain manufactures will rejoice to be informed that a mine of very excellent cobalt has been discovered in this country. We have been informed by Mr. Hume of Long-Acre, to whom a specimen has been submitted for examination, that on analysis he has found it to contain nearly 30 per cent. of that metal.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Bayley, of Birmingham, for certain improvements in sliding pulleys for window blinds, and for other purposes.—January 15.

To Peter Cox, of Fairford, in the county of Gloucester, civil engineer, for certain improvements upon the thrashing machine.—January 23.

To Joseph Manton, of Davies-street, Berkeley-square, gun-maker, for an improvement in telescopes.—Jan. 23.

To David Cock, of Dean-Street, Soho, in the county of Middlesex, stereotype manufacturer, for vessels of a new construction, for melting and heating fluids.—Feb. 1.

To Augustus Frederick de Heine, of Moor Lane, Fore-Street, in the city of London, gent., for certain improvements on printing and stamping-presses.—Feb. 1.

To John Craigie, of Quebec, in our province of Lower Canada, in North America, esq., now residing in Craven Street, in the county of Middlesex, who in consequence of communications made to him when residing abroad, and certain inventions by himself, is in possession of a method of making an improved kitchen fire-place.—Feb. 1.

To Stedman Adams, in the city of Hartford, in the state of Connecticut, in North America, esq., at present residing in Carey Street, Lincoln's Inn Fields, in the county of Middlesex, for certain improvements on steam-engines, and in distillation.—Feb. 1.

To William Muller, of the Hay Market, in the county of Middlesex, for certain improvements in the construction of pumps.—Feb. 12.

To John Slater, of Birmingham, in the county of Warwick, coach-spring maker, for an improvement in hanging and securing grind stones from breaking in the middle or centre.—Feb. 12.

To William Doughty, of Birmingham, in the county of Warwick, engineer, for his improvement in the combination of wheels for gaining mechanical power.—Feb. 12.

To George Wyke, esq., of the city of Bath, for certain improve-

improvements in the construction of wheel carriages of various descriptions.—Feb. 12.

To Peter Warburton, of Cerbridge, in the county of Stafford, china manufacturer, for his new method of decorating china, porcelain, earthenware, and glass, with native pure or adulterated gold, silver, platina, or other metals, or fluxed, or lawered with lead, or any other substance; which invention leaves the metals after being burned in their metallic state.—Feb. 13.

To Richard Witty, of the town of Kingston-upon-Hull, gent., for certain improvements in making, arranging and combining certain parts of rotative steam-engines, by which means the most complex parts of the steam-engines now in use are dispensed with and rendered unnecessary, and the whole of the mechanism made much more simple, less expensive, and not so liable to be out of repair, as that of the steam-engines now in use, and applicable to giving motion to all sorts of mill-work or machinery.—Feb. 14.

To Eneas Morrison of the town of Greenock in Scotland, for a machine for conveying persons from the upper parts of houses on fire, and for lowering goods from warehouses, and other purposes.—Feb. 22.

To Peter Stuart, late of Fleet Street, in the city of London, printer, for his method of engraving and printing maps of counties, charts, or other plans or designs, music, mathematical diagrams or figures on wood, metal, or any other substance, so that they may be thrown off in a common printing press or presses, either for books, newspapers, or any other printed paper whatever.—Feb. 26.

To William Bainbridge, of the parish of St. Andrew Holborn, in the city of London, musical instrument maker, for certain improvements on the English flute and flageolet.—Feb. 26.

To Major Pratt, of Spencer Street, St. George's-in-the-East, in the county of Middlesex, farmer, for certain methods of manufacturing machines for performing various agricultural operations by mechanical powers.—Feb. 26.

To Augustus de Heine, of Burr Street, in the county of Middlesex, gent., for certain apparatus by the application of known principles to preserve animal food, vegetable food, and other perishable articles, a long time from perishing or becoming useless.—Feb. 26.

To Charles le Caan, of the town of Llanelly, in the county of Carmarthen, gent., for certain apparatus to be added and united to the axle-trees and wheels or naves of wheels of carriages, so as to impede, resist, or check their action.—Feb. 26.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For February 1810.

Days of the Month.		Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan.	27	32	32°	31°	30·21	0	Cloudy
	28	31	33	30	·22	7	Cloudy
	29	30	33	30	·28	6	Cloudy
	30	33	33	30	·40	4	Cloudy
	31	32	43	47	·25	0	Cloudy
Feb.	1	47	47	46	·02	0	Rain
	2	46	47	44	29.90	0	Rain
	3	45	47	46	·70	0	Rain
	4	45	46	36	·92	10	Fair
	5	33	45	44	30·00	10	Fair
	6	46	47	44	29·96	7	Fair
	7	47	47	46	·94	4	Cloudy
	8	46	47	45	·90	0	Rain
	9	44	47	46	·70	0	Rain
	10	47	50	44	·78	0	Rain
	11	44	44	40	·81	0	Rain
	12	40	41	40	·42	0	Rain
	13	38	42	38	28·95	6	Stormy
	14	36	39	36	29·50	5	Foggy
	15	35	37	30	·90	7	Cloudy
	16	26	38	27	30·00	10	Fair
	17	27	35	26	·10	9	Fair
	18	27	33	36	·11	0	Snow
	19	28	36	28	29·98	5	Fair
	20	27	30	24	30·22	7	Fair
	21	19	30	26	·40	10	Fair
	22	26	34	28	·08	5	Fair
	23	35	45	40	29·40	0	Rain
	24	40	49	47	·50	0	Small rain
	25	47	51	40	·46	35	Fair
	26	37					

N. B. The Barometer's height is taken at one o'clock.

XXV. *On Injuries of the Brain.*

To Mr. Tilloch.

Sir, In the last Number of your Magazine, there is a very interesting case recorded of considerable derangement of the structure of the brain, thoracic and abdominal viscera. —The former it is my intention particularly to advert to.

The person in whom this disease (tumours in the medullary substance of the brain) existed, is noticed to have been an acute reasoner, a man of good understanding:—in short, to use the author's own words, "distinguished for the facility with which he could converse upon most subjects; and reasoned so closely that his intellectual powers were generally regarded as of a superior kind."

Lately I examined the brain of a person who died of insanity. It was a female between 60 and 70 years of age, who, as far as I could learn, had been deranged for some considerable time (at least 10 years); but, as I had not an opportunity of seeing her whilst alive, cannot distinctly say of what species:—suffice it to observe, it was of the raving kind.

Upon removing the calvaria (or upper part of the cranium), and raising the dura mater, I discovered the tunica arachnoides to be very opake; I could easily distinguish it upon the superior surface of the brain, which cannot be done with facility in the natural state, or where no disease is present. The vessels of the pia mater were loaded with blood, but no effusion was apparent between the membranes. Upon separating the hemispheres to observe the corpus callosum, I found the arteriæ callosæ considerably enlarged, and in that state which precèdes ossification, and there was no appearance of raphè. I then proceeded to remove part of the left hemisphere, in order more readily to examine the ventricles; in doing which I nearly cut through the whole of a *tumour*, which I found to be situated partly in the cineritious and partly in the medullary substance of the brain, opposite to the temporal fossa.

The tumour was about the size of a half-crown piece, and somewhat of that shape, though inclining to oval; it was of a granulated appearance, highly vascular, and around the edge presented a dark blue colour; it was in structure *precisely* similar to those (for I had an opportunity of examining them in the recent state) described by Mr. Taunton;—there was an artery entering at the outer side which was in a state of ossification.

It is necessary to observe, that the substance of the brain was of a very soft consistence, whereas in the case related by Mr. T. it was of a peculiar hardness. I do not imagine that this softness arose from the mental derangement; as it has occurred to my lot to examine the brain of many persons who have died insane, where this organ has been of its usual texture, and sometimes unusually firm.

Many other diseased appearances were observed during the dissection, but which are generally attendant in cases of mania; such as water in the ventricles, opacity of the septum lucidum, bloody points in the medullary substance of the brain, &c. besides opakeness of the tunica arachnoides, and turgescence of the vessels of the pia mater before mentioned.

The whole of the nerves arising from the brain were uncommonly firm, and the olfactory (as in the instance recorded by Mr. T.) were in appearance similar to a piece of narrow tape, adhering strongly to the cribriform process of the ethmoidal bone, and afforded some little resistance to the knife.

Mr. T.'s case appears to establish the fact, of the brain, the most delicate organ in the whole structure of man, being capable of accommodating itself to an extraneous substance without producing any visible alteration in the operations of the mental faculties. The question which seems naturally to arise from this circumstance is: Whether the substance of the brain was not absorbed in proportion to the quantity of deposition secreted by the arteries?—and, Whether this could be effected sufficiently gradually, not to impede the functions of that wonderful and anomalous organ*? placed (as Harwood elegantly expresses it) on the doubtful confines of the material and spiritual worlds!

That the brain may become absorbed in proportion to the growth of the tumours, appears to me highly probable; and that these tumours (in Mr. T.'s case at least, if not in mine) were in the first instance exudations of lymph, which in course of time became organized. It is an interesting subject, and I hope will be considered by more able anatomists and physiologists than myself.

With respect to the abdominal viscera:—That the *pancreas* as well as many other of the organs contained in the abdomen may be diseased and not suspected, I have witnessed in several instances. I have in a number of cases

* That the arteries performed this secretion in a very slow manner must be obvious, as no symptoms of compression were present during life.

after

after death upon examination found that organ in a considerable state of disease, where the only symptom that existed during life was a slight degree of dyspepsia.

I am yours. &c.

Fleet Street, Feb. 10, 1810. T. J. Pettigrew.

XXVI. *On Salmon-Leaps.*

To Mr. Tilloch.

Sir, Whoever carefully peruses Mr. Carr's paper "On the ascent of salmon over the elevations in the course of rivers called salmon-leaps," as given in your Magazine for November last, must needs be astonished indeed at the rapidity of the growth of young salmon, from the period of their being spawned to their departure out of the rivers. As he is not precise as to the time when these occurrences happen, I must beg leave to state them:—The spawn is deposited chiefly in December and January, and the salmon depart in the beginning of April, being on an average a space of about fourteen weeks. Now admitting the eggs to be batched in eight weeks (which I believe is much too little), we have only six weeks for the young fry to arrive at the length of *six or eight inches;* an increase which is absolutely incredible, more especially when we consider the comparative *want* of food incident to the season.

The fact is; the young fry do not descend the rivers with the old salmon, in the spring after they are spawned; for in the month of October following they are no bigger than a minnow. Mr. John Clayton of Stockport (who is reckoned to be one of the most experienced anglers in the kingdom), and others, have frequently caught them of the size, and at the time stated, and are fully satisfied of their being young salmon. In the months of June and July they are caught about five or six inches in length: this I know to be fact; and it is not till the ensuing spring that they pass with the old ones down the rivers with the floods into the sea. Their growth is there very rapid, as they are found on their return, in the months of August and September, to weigh from 14 to 20 ounces.

This statement, if not established beyond all doubt by incontrovertible facts, is highly probable, and accords more with their progressive growth, and rational conjecture, than the account given to us by Mr. Carr. I am, &c.

Feb. 10, 1801. Piscator.

 XXVII. *On*

XXVII. *On Platina and Native Palladium from Brasil.* *By* William Hyde Wollaston, *M.D. Sec.R.S.**

Although platina has now been known to mineralogists for more than 60 years, yet it had not been discovered in any other places than Choco and Santa Fé, whence it was originally brought, until about two years since M. Vauquelin discovered it in some gray silver ores from Guadalcanal in Estremadura. In analysing these ores, he found some fragments that contained as much as one-tenth of their weight of platina, but he did not find it accompanied by any of the new metals that have lately been discovered in the Peruvian ore of platina.

The specimen which I am now about to describe is derived from a third source, and it is rendered the more interesting by having grains of native palladium mixed with it. This new mineral has lately been received from the gold mines in Brasil, by H. E. Chev. de Souza Coutinho, ambassador from the court of Portugal, resident in this country; and I am in hopes that some account of it may be acceptable to the Royal Society, although the analysis must necessarily be very imperfect, from the small quantity to which my experiments have unavoidably been confined.

The general aspect of this specimen is so different from the common ore of platina, that I could form no conjecture of what ingredients it might be found to consist. Its appearance was such indeed, as at first sight to induce a suspicion of its not being in a natural state, for it had very much the spongy form which is given to platina from imperfect attempts to render it malleable by means of arsenic.

One circumstance, however, occasions a presumption that no art has been employed in giving the grains their present appearance; as upon close inspection many small particles of gold are discernible, but there is none of the magnetic iron sand with which the Peruvian ore abounds, nor any of the small hyacinths, which I have formerly noticed as accompanying that mineral†.

It is very well known, that the common ore of platina in general consists of flattened grains, that appear so much worn at their surface, as to be in a considerable degree polished, and the roughness observable in some of the larger grains arises from concave indentations of a reddish brown

* From Philosophical Transactions for 1809, Part II.
† Phil. Trans. for 1805, p. 318.

or black colour. The Brasilian platina, on the contrary, has no polish, and does not appear worn; but most of the grains seem to be small fragments of a spongy substance, and even those which are yet entire aud rounded on all sides, present a sort of roughness totally different from that of the former, as their surface consists of small spherical protuberances closely coherent to each other, with the interstices extremely clean, and free from any degree of tarnish.

The first portion that I employed for solution was taken without any selection, and being digested with a small quantity of nitro muriatic acid, two of the grains were acted on much more rapidly than is usual with platina, and seemed to give a redder colour than that metal alone. These grains were consequently taken out, washed, and reserved for separate examination, and the solution was allowed to proceed till the rest were entirely dissolved. By the addition of muriate of ammonia an abundant precipitate was formed of a bright yellow colour. This precipitate was evidently platina, and its colour satisfied me that the grains had not been brought into their present state from Peruvian platina by means of arsenic; for where arsenic has been employed, I have observed that the iridium contained in that ore is rendered more soluble than before, and hence communicates its red colour to the precipitate.

From the grains thus examined, there appeared not to be any iridium dissolved, nor any black powder containing iridium undissolved.

I next endeavoured, by prussiate of mercury, to ascertain the presence of palladium; but though a precipitate which occurred indicated a certain quantity, it remained doubtful whether it was derived from the grains of platina themselves, or from the two small fragments that had been in part dissolved before they were separated from the rest.

By addition of ammonia to the solution, no iron was precipitated; and when the solution was afterwards allowed slowly to evaporate, I could discern no crystals or colour that I could ascribe to the presence of rhodium. In short, it seemed that these grains are really native platina nearly pure.

In order to discover whether the grains themselves contained any portion of gold, I selected three of the largest, weighing together eight grains and a half; and after a solution and precipitation, as before, by muriate of ammonia, I added a solution of green sulphate of iron, and obtained a precipitate of gold. It was, however, far too small in quantity

tity to be estimated with correctness, but certainly did not exceed the $\frac{1}{200}$ of a grain. This, it is to be observed, is another circumstance in which the present mineral differs from the Peruvian ore of platina, which I believe never contains (in the ore itself) the smallest quantity of gold.

In this experiment also, I tried to detect the existence of palladium in the solution, and by prussiate of mercury again ascertained its presence; but it was in too small quantity for estimating the proportion it bore to the whole mass.

It may deserve to be remarked, that though neither the Peruvian nor Brasilian grains of platina contain any silver, yet the gold which accompanies them is in each instance so much alloyed with silver, that from about thirty small scales of gold picked from Peruvian platina, weighing two grains, I obtained as much as four tenths of a grain of silver, or one fifth part of their weight.

Native Palladium.

The two fragments, that had been separated from the first solution, next claimed my attention, and evidently deserved a careful examination. They were each placed in a drop of nitric acid, and each communicated a deep red colour, which, by the tests of prussiate of mercury and green sulphate of iron, I was satisfied arose from palladium. The smaller fragment was then divided, and one portion allowed to remain in the acid till it seemed completely dissolved, and the other examined by the blow-pipe. The utmost heat that could be given, appeared to have no effect: but when a small piece of sulphur was applied to it, it fused instantly; by continuance of the heat, it parted with the sulphur, and became completely malleable. In short, it perfectly resembled palladium; and as it retained its brilliancy in cooling, I judged it to be nearly pure.

But as the surfaces which had been acted upon by nitric acid had a degree of blackness, that might be owing to some insoluble impurity, I have since that time dissolved the larger fragment for the sake of discovering the cause of this appearance. Hot nitric acid dissolved by far the greatest part; but there remained a black powder on which a fresh addition of this acid alone had no further effect. But when a drop or two of muriatic acid was added, the whole was very soon dissolved. By the addition of muriate of ammonia, it became evident from the precipitate that the residuum was principally platina. But this precipitate, instead of being yellow, had the deep red colour, which is usually occasioned by the presence of iridium. The pla-

tina

tina reduced from this precipitate was also too black for pure platina, and when it was again dissolved, the solution was of a deep red, and the precipitate by muriate of ammonia red, as before; so that although the grains of Brasilian platina appear to be free from iridium, as well as from many other impurities that form part of the Peruvian ore, yet the grains of native palladium that accompany them, afford a trace of this ingredient, and occasion a presumption that osmium and rhodium may hereafter appear, when we can obtain this mineral in larger quantity.

Since the whole weight of metal employed in the last experiment did not exceed $1\frac{2}{10}$ grain, it is in vain to attempt to estimate the proportion of the ingredients; but if I am near the truth, in considering the quantity of the red precipitate as about one fifth of a grain, of which less than half is platina, those who are best acquainted with the intense colouring power of iridium may endeavour to form a conception of the extremely small quantity that can be present.

As soon as I had ascertained the existence of native palladium, I endeavoured, by examination of its external characters, to distinguish its appearance from that of the surrounding substances, and I found it by no means difficult, although no difference of colour could be discerned. Having remarked that the larger fragment appeared rather fibrous, and that the fibres were in some degree divergent from one extremity, I examined the remainder of the small specimen which had originally been given to me, and by this peculiarity of structure I soon detected a third fragment, which upon trial proved to be the same substance. By favour of the Chev. de Souza I was also permitted, with this view, to examine the specimen which remained in his possession, and had soon the satisfaction of discovering two more fragments of the same mineral; and as I was in no one instance deceived in my choice, by attending to the radiating fibres, I am in hopes that this external character will enable persons to distinguish that metal, in situations where they have not an opportunity of deciding by chemical experiment.

 XXVIII. *On*

XXVIII. *On an Improvement in the Manner of dividing astronomical Instruments.* By Henry Cavendish, *Esq., F.R.S.**

The great inconvenience and difficulty in the common method of dividing, arises from the danger of bruising the divisions by putting the point of the compass into them; and from the difficulty of placing that point mid-way, between two scratches very near together, without its slipping towards one of them; and it is this imperfection in the common process, which appears to have deterred Mr. Troughton from using it, and thereby gave rise to the ingenious method of dividing described in the preceding part of this volume†. This induced me to consider, whether the above-mentioned inconvenience might not be removed, by using a beam compass with only one point, and a microscope instead of the other; and I find, that in the following manner of proceeding, we have no need of ever setting the point of the compass into a division, and consequently that the great objection to the old method of dividing is entirely removed.

In this method, it is necessary to have a convenient support for the beam compass: and the following seems to me to be as convenient as any. Let C C C (Plate V. Fig. 1.) be the circle to be divided, B B B a frame resting steadily on its face, and made to slide round on it with an adjusting motion to bring it to any required point: *dδ* is the beam compass, having a point near *δ*, and a microscope *m* made to slide from one end to the other. This beam compass is supported at *d*, in such manner as to turn round on this point as a centre, without shake or tottering; and at the end *δ* it rests on another support, which can readily be lowered, so as either to let the point rest on the circle, or to prevent its touching it. It must be observed, however, that as the distance of *d* from the centre of the circle must be varied, according to the magnitude of the arch to be divided, the piece on which *d* is supported had best be made to slide nearer to, or further from, the centre; but the frame must be made to bear constantly against the edge of the circle to be divided, so that the distance of *d* from the centre of this circle, shall not alter by sliding the frame.

This being premised, we will first consider the manner of dividing by continued bisection. Let F and *f* be two

* From Philosophical Transactions for 1809, Part II.

† See Phil. Mag. vol. xxxiv. pages 81 and 163.

points

points on this limb which are to be bisected in ϕ. Take the distance of the microscope from the point nearly equal to the chord of $f\phi$, and place d so that the point and the axis of the microscope shall both be in the circle in which the divisions are to be cut. Then slide the frame B B B till the wire of the microscope bisects the point F; and having lowered the support at δ, make a faint scratch with the point.

Having done this, turn the beam compass round on the centre d till the point comes to D, where it must rest on a support similar to that at δ; and having slid the frame till the wire of the microscope bisects the point f, make another faint scratch with the point, which, if the distance of the microscope from the point has been well taken, will be very near the former scratch; and the point mid-way between them will be the accurate bisection of the arch Ff; but it is unnecessary, and better not to attempt to place a point between these two scratches.

Having by these means determined the bisection at ϕ, we must bisect the arches F ϕ and $f\phi$ in just the same manner as before, except that the wire of the microscope must be made to bisect the interval between the two faint scratches, instead of bisecting a point.

It must be observed that when the arch to be bisected is small, it will be necessary to use a bent point, as otherwise it could not be brought near enough to the axis of the microscope; and then part of the rays, which form the image of the object seen by the microscope, will be intercepted by the point; but I believe, that by proper management this may be done without either making the point too weak, or making the image indistinct; but if this cannot be done, we may have recourse to Mr. Troughton's expedient of bisecting an odd number of contiguous divisions.

It must be observed too, that in the bisections of all the arches of the same magnitude, the position of the point d on the frame remains unaltered; but its position must be altered every time the magnitude of the arch is altered.

It is scarcely necessary to say, that the bisections thus made are not intended as the real divisions, but only as marks from which they are to be cut. In order to make the real divisions, the microscope must be placed near the point, and the support d must be placed so that $d\ \delta$ shall be a tangent to the circle at δ. The wire of the microscope must then be made to bisect one of these marks, and a point or division cut with the point, and the process continued till the divisions are all made.

It

It is plain that in this way, without some further precaution, we must depend on the microscope not altering its position in respect of the point during the operation; for which reason I should prefer placing the axis of the microscope at exactly the same distance from the centre of motion *d*, as the point; but removed from it sideways, by nearly the semi-diameter of the object glass; so that having made the division, we may move the beam compass till the division comes within the field of the microscope, and then see whether it is bisected by the wire, and consequently see whether the microscope has altered its place.

In the operation of bisection, as above described, it may be observed, that if the two scratches are placed so near together, that in making the second the point of the compass runs into the burr raised by the first, there seems to be some danger that the point may be a little deflected from its true course; though in Bird's account of his method, I do not find that he apprehends any inconvenience from it. One way of obviating this inconvenience, if it does exist, would be to set the beam compass not so exactly to the true length, as that one scratch should run into the burr of the other; but as this would make it more difficult to judge of the true point of bisection, perhaps it might be better to make one scratch extend from the circle towards the centre, and the other from it.

It is clear, that the entire arc of a circle cannot be divided to degrees, without trisection and quinquesection; and I do not know whether our artists have recourse to this operation, or whether they avoid it by some contrivance similar to Bird's; namely, that of laying down an arch capable of continued bisection; but if the method of quinquesection is preferred, it may be performed by either of the three following methods:

First Method.

Let $a\ \alpha$ (Fig. 2) be the arch to be quinquesected. Open the beam compass to the chord of one fifth of this arch; bring the microscope to a, and with the point make the scratch f; then bring the microscope to f, and draw the scratch e; and in the same manner make the scratches d and b. Then turn the beam compass half round, and having brought the microscope to α, make the scratch β; and proceeding as before, make the scratches δ, ε and φ. Then the true position of the first quinquesection will be between b and β, distant from β by one fifth of $b\ \beta$, and the second will be distant from δ by two fifths of $d\ \delta$, and so on.

Then, in subdividing these arches, and striking the true divisions,

divisions. the wire of the microscope, instead of bisecting the interval between the two scratches, must be brought four times nearer to β than to b. But in order to avoid the confusion which would otherwise proceed from this, it will be necessary to place marks on the limb opposite to all those divisions, in which the interval of the scratches is not to be bisected, showing in what proportion they are to be divided; and these marks should be placed so as to be visible through the microscope, at the same time as the scratches. Perhaps, the best way of forming these marks, would be to make dots with the point of the beam compass contiguous to that scratch which the wire is to be nearest to, which may be done at the time the scratch is drawn.

Perhaps an experienced eye might be able to place the wire in the proper manner, between the two scratches, without further assistance; but the most accurate way would be to have a moveable wire with a micrometer, in the focus of the microscope, as well as a fixed one; and then having brought the fixed wire to b, bring the moveable one to β, and observe the distance of the two wires by the micrometer; then reduce the distance of the two wires to one fifth part of this, and move the frame till the moveable wire comes to β, and then the fixed wire will be in the proper position, that is four times nearer to β than to b.

It will be a great convenience, that the moveable wire should be made in such manner, as to be readily distinguished from the fixed, without the trouble of moving it.

In this manner of proceeding, I think a careful operator can hardly make any mistake: for if he makes any considerable error in the distance of the moveable wire from the fixed, it will be detected by the fixed wire not appearing in the right position, in respect of the two scratches; and as the mark is seen through the microscope, at the same time as the scratches, there is no danger of his mistaking which scratch it is to be nearest to, or at what distance it is to be placed from it.

To judge of the comparative accuracy of this method with that of bisection, it must be considered that the arches $\alpha\beta$, $\beta\delta$, &c. though made with the same opening of the compass, will not be exactly alike, owing partly to irregularities in the brass, and partly to other causes. Let us suppose, therefore, that in dividing the arch $a\,\alpha$ into five parts, the beam compass is opened to the exact length, but that from the abovementioned irregularities the arches $\alpha\beta$, $\beta\delta$, $\delta\epsilon$, and

and $\varepsilon\phi$ are all too long by the small quantity ε, and that the arches af, fe, ed, and db are all too short by the same quantity, which is the supposition the most unfavourable of any to the exactness of the operation; then the error in the position of $\beta = \varepsilon$, and the point b errs 4ε in the same direction, and therefore the point assumed as the true point of quinquesection, will be at the distance of $\frac{3\varepsilon}{5}$ from β, and the error in the position of this point $= \varepsilon \times 1\frac{3}{5}$.

By the same way of reasoning, the error in the position of the point taken between d and $\delta = \varepsilon \times 2\frac{2}{5}$.

In trisecting the error of each point $= \varepsilon \times 1\frac{1}{3}$; and in bisecting, the error $= \varepsilon$; and in quadrisecting, the error of the middle point $= 2\,\varepsilon$.

It appears therefore that in trisecting, the greatest error we are liable to does not exceed that of bisection in a greater proportion than that of 4 to 3; but in quinquesecting the error of the two middle points is $2\frac{2}{5}$ times greater than in bisecting. It must be considered, however, that in the method of continued bisection, the two opposite points must be found by quadrisection; and the error of quinquesection exceeds that of quadrisection in no greater proportion than that of six to five; so that we may fairly say, that if we begin with quinquesection, this method of dividing is not greatly inferior, in point of accuracy, to that by continued bisection.

Second Method.

This differs from the foregoing, in placing dots or scratches in the true points of quinquesection and trisection, before we begin to subdivide. For this purpose, we must have a microscope placed as in page 170, first par. at the same distance from the centre of motion as the point is; and this microscope must be furnished with a moveable wire and micrometer, as in page 171; and then having first made the fixed wire of this microscope correspond exactly with the point, we must draw the scratches b and β, d and δ, &c. as before, and bring the fixed wire to the true point of quinquesection between b and β, in the manner directed in page 226, and with the point strike the scratch or dot: and if we please, we may, for further security, as soon as this is done, examine, by means of the moveable wire, whether this intermediate scratch or dot is well placed.

The advantage of this method is, that when this is done, we may subdivide and cut the true divisions, by making the wire of the microscope bisect the intermediate scratches, instead of being obliged to use the more troublesome operation

ration of placing it in the proper proportion of distance between the two extremes.

This method certainly requires less attention than the former, and on the whole seems to be attended with considerably less trouble; but it is not quite so exact, as we are liable to the double error of placing the intermediate point and of subdividing from it.

As in this method the intermediate points are placed by means of the micrometer, there is no inconvenience in placing the extreme scratches b and β, &c. at such a distance from each other, that the intermediate one shall be in no danger of running into the burr raised by the extremes.

Third Method.

Let $a\ \alpha$ (Fig. 3.) be the arch to be quinquesected; lay down the arches $a\ b$, $d\ b$, and $d\ e$, as in the first method; then turn the beam compass half round, and lay down the arches $\alpha\ \beta$ and $\beta\ \delta$; then, without altering the frame, move the moveable wire of the microscope till it is four times nearer to δ than to e, and, having first rubbed out the former scratches, lay them down again with the compass thus altered: but as this method possesses not much, if any, advantage over the second, in point of ease, and is certainly inferior to it in exactness, it is not worth while saying any thing further about it.

It was before said*, that the centre of motion of the beam compass is to be placed, so that the point and axis of the microscope shall both be in the circle in which the divisions are made; but it is necessary to consider this more accurately. Let Aδ (Fig. 4.) be the circle in which the scratches are to be made, δ the point of the beam compass, which we will suppose to be exactly in this circle, d the centre on which it turns, and Mm the wire in the focus of the microscope, and let m be that point in which it is cut by the circle; and let us suppose that this point is not exactly in the line $d\ \delta$, then, when the beam compass is turned round, the circle will cut the wire in a different point μ, placed as much on one side of $d\ \delta$, as m is on the other, so that if the wire is not perpendicular to $d\ \delta$, the arch set off by the beam compass, after being turned round, will not be the same as before; but if it is perpendicular, there will be no difference; for which reason, care should be taken to make the wire exactly perpendicular to $d\ \delta$, which is easily examined by observing whether a point appears to run along

* Page 168.

it,

it, while the beam compass is turned a little on its centre. It is also necessary to take care that the point δ is in the arc of the circle, while the bisection is observed by the microscope, which may most conveniently be obtained, by placing a stop on the support on which that end of the beam compass rests. If proper care, however, is taken in placing the wire perpendicular, no great nicety is required either in this or in the position of *d*.

Another thing to be attended to, in making the wire bisect two scratches, is to take care that it bisects them in the part where they cut the circle; for as the wire is not perpendicular to the circle, except in very small arches, it is plain, that if it bisects the scratches at the circle, it will not bisect them at a distance from it.

There are many particulars in which my description of the apparatus to be employed will appear incomplete; but as there is nothing in it which seems attended with difficulty, I thought it best not to enter further into particulars, than was necessary to explain the principle, and to leave the rest to any artist who may choose to try it.

It is difficult to form a proper judgement of the conveniences or inconveniences of this method, without experience; but, as far as I can judge, it must have much advantage, both in point of accuracy and ease, over that of dividing by the common beam compasses: but it very likely may be thought that Mr. Troughton's method is better than either. Whether it is or is not, must be left for determination to experience and the judgement of artists. Thus much, however, may be observed, that this, as well as his, is free from the difficulty and inaccuracy of setting the point of a compass exactly in the centre of a division. It also requires much less apparatus than his, and is free from any danger of error, from the slipping or irregularity in the motion of a roller; in which respect his method, notwithstanding the precautions used by him, is perhaps not entirely free from objection; and, what with some artists may be thought a considerable advantage, it is free from the danger of mistakes in computing a table of errors, and in adjusting a sector according to the numbers of that table.

XXIX. *On*

XXIX. *On* M. Bemetzrieder's *erroneous Calculations of the Magnitudes of certain musical Intervals. By* Mr. John Farey.

To Mr. Tilloch.

Sir, When I some time ago took up my pen*, to combat the erroneous principles advanced by *Earl Stanhope*, respecting the accuracy of expressing musical intervals by the *difference* of the *lengths* of strings producing the sounds, I thought that his lordship had an exclusive claim to this and other similar and "important musical truths," which he has advanced; but a folio work opening the long-way was this day put into my hands, entitled, "General Instructions in Music," by M. Bemetzrieder, printed at London, about the year 1780 (as I have been told), price one guinea. Who after stating, at page 92, that a major tone $\frac{8}{9}$ taken from a minor fourth $\frac{3}{4}$, or $\frac{3}{4} \times \frac{9}{8}$ produces $\frac{27}{32}$ (which he calls a minor third) and which taken from a fifth $\frac{2}{3}$, or $\frac{2}{3} \times \frac{32}{27}$ produces $\frac{64}{81}$ (which he calls a major third), and mentioning, that $\frac{5}{6}$ and $\frac{4}{5}$ are ratios *usually* assigned to the 3d. and IIId., thereon subjoins these remarks: "It is "probable, that the *facility of arithmetical calculation* has "been preferred to geometrical exactitude: besides, the "difference of the two (major) thirds $\frac{4}{5}$ and $\frac{64}{81}$ is no more "than $\frac{4}{405}$ $\left(\text{being the } \textit{difference} \text{ of } \frac{324}{405} \text{ and } \frac{320}{405}\right)$ the 90th "part of a tone $\left(\text{because } \frac{4 \times 90}{405} = \frac{360}{405} = \frac{8}{9}\right)$ which is *better* "*perceived by the understanding than by the ear*; the "(major) third $\frac{4}{5}$ is a 90th part of a tone lower than the "third $\frac{64}{81}$. The difference of the two minor thirds $\frac{5}{6}$ "and $\frac{27}{32}$ is $\frac{1}{96}$; the (minor) third $\frac{5}{6}$, is $\frac{1}{96}$ too acute:" for as above, $\frac{162}{192} - \frac{160}{192} = \frac{2}{192} = \frac{1}{96}$, and still proceeding as above, $\frac{1 \times 83\frac{1}{3}}{96} = \frac{8}{9}$; or, $\frac{27}{32}$ is more than $\frac{5}{6}$ by an $83\frac{1}{3}$ part of the same tone $\left(\frac{8}{9}\right)$ as above. Now, if instead of taking the *numerical differences* of the thirds, in Mr. B.'s erroneous

* See our 27th Volume, page 193, and 33d Volume, page 294 and 5.—Editor.

method,

method, the *ratios* had been subtracted thus, $\frac{64}{81} \times \frac{5}{4} = \frac{320}{324} = \frac{80}{81}$, and $\frac{5}{6} \times \frac{32}{27} = \frac{160}{162} = \frac{80}{81}$ the results would be the major comma in both instances, instead of one being a 90th, and the other an $83\frac{1}{3}$rd part of a tone, neither of which are much different from one ninth part of what they *ought to be*, being altogether the produce of egregious blundering; for the major comma is known to be nearly the $9\frac{1}{2}$th part of a major tone, or more exactly, $9{\cdot}48141 \times c = T$. And thus we see, that the error of a whole comma is said by this gentleman not to be perceivable by the ear! and guided as I suppose by the above erroneous principle, he has added to the confusion which already reigned, as to the *names* of intervals, by giving many new and absurd ones; for instance, he calls $\frac{16384}{19683}$ a superfluous *second*, although it exceeds a minor *third* in magnitude, $\frac{59049}{65536}$ he calls a diminished *third*, although the same is less than a minor tone, or comma-deficient major *second*, and so of many others. At page 116 he asserts, that 1, $\frac{1}{3}$ and $\frac{1}{5}$ are not the Harmonicals or "replies" of a sound, but 1, $\frac{1}{3}$ and $\frac{16}{81}$!

Hoping, Sir, that this will be the last time that I shall have to expose such gross ignorance in authors, on the nature and fundamental principles of musical calculation,

I remain yours, &c.

Westminster, 12th March 1810. JOHN FAREY.

XXX. *On the Culture of Parsnips, and their Utility in feeding Cattle.* By CHARLES LE HARDY, *Esq., of the Island of Jersey**.

SIR, HAVING observed in the book of Premiums offered by the Society, that they wished for information on the culture of parsnips, which are much used in the island of Jersey;—as having practised it for many years, I take the liberty to communicate what I know on the subject, with the result of some comparative experiments.

The culture of parsnips and beans is looked upon as one of the regular courses of crops in the island. There is no farmer, be the extent of his grounds ever so small, who

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1809.——The Society voted their silver medal to Mr. Le Hardy for this communication.

does

does not yearly plant a proportionate quantity, for the purpose of fattening his hogs and cattle, or of feeding his milch cows.

A few years ago, the culture of potatoes was substituted by some farmers to that of parsnips, and apparently with advantage; but further experience has brought them back again to their former practice. Potatoes produce more weight and measure on a given extent of ground, and may be cultivated with less expense; still the parsnip is found to answer best for the farmer's purpose. A perch of the island, which is twenty-four square feet, will produce on an average crop, seven cabots of potatoes, each weighing forty pounds; the same extent in parsnips will only average six cabots, which weigh only thirty-five pounds each, making twenty pounds weight in favour of the potatoes;—but they are not so nutritious as parsnips.

Parsnips will thrive almost any where, but better in a deep stiff loam. They are generally cultivated in the island after a crop of barley, in the following manner.—At the end of January or the beginning of February, the soil, which requires for that purpose to be stirred from the bottom, is either dug with spades after a skimming plough, or with two ploughs of different shapes following one another. The latter of the two, invented some years ago by a farmer in the island, will go to a depth of fifteen inches. In both these ways the neighbouring farmers assist each other: in the season, it is not uncommon to see forty or fifty men in one field digging after a plough. When the large plough is used, less men are required, but more strength of cattle; two oxen and six horses are the team generally used. Those days are reckoned days of recreation, and tend to promote social intercourse among that class of men.

After the ground has been tilled in this way, it is coarsely harrowed, and a sufficient number of women are provided to plant beans. These are dibbled in rows three by three ⁝⁝⁝⁝⁝⁝⁝⁝ at the distance of five feet from row to row. Two women may plant one vergee in a day: two vergees and a half being equal to an English acre. Three sixtenniers of parsnip seed (about $\frac{1}{4}$ of a Winchester bushel) are then sown upon each vergee, and the whole is finely harrowed.

This crop now requires no attendance till the month of May, when weeding becomes necessary. This is the most expensive part of the culture. It is generally done by hand, with a small weeding fork; and as the parsnips require to be kept very clean, the expense is proportionate to the quantity of weeds. This last summer four women were em-

 ployed

ployed twenty-eight days each in weeding about five vergees. I tried a few perches with the hand-hoe, and thinned them like turnips; they proved finer than those which were hand-weeded. In Guernsey they make use of the spade for that purpose.

In the beginning of September the beans are pulled up from among the parsnips, and about the latter end the digging begins. The instrument used is the common three-pronged fork. This work is done gradually as the cattle want them, till the ground requires to be cleared for sowing wheat, which after parsnips is generally done about the middle of December. They are reckoned an excellent fallow for that kind of grain, and the finest crops are generally those which succeed them: as it is a tap-rooted plant, it does not, like the potatoe, impoverish the surface, but leaves it mellow and free from weeds, to a succeeding crop.

When parsnips require to be kept for the use of cattle, they are brought dry under sheds, and will keep good without any care till the end of March. Should they require to be kept longer, they are laid in double rows over one another, their heads outward, with alternate strata of earth, which, when finished, have the appearance of small walls, or, if made circular, of small towers. Those for seed are always preserved in this manner, and sometimes carrots and beets for culinary purposes.

Parsnips are not injured by frost; after having been frozen, they are fit for vegetation: the only sensible alteration is their acquiring a sweeter taste, and by that perhaps becoming more nutritive. They are given raw to hogs and to horned cattle. Though horses are fond of these roots, they are not suffered to eat them, as they make them languid, and are apt to injure their sight. Their leaves when wet are so caustic as to blister the hands of the weeders, and sometimes to occasion a violent inflammation in the eyes and udders of the cattle feeding upon them.

Cows fed on parsnips in the winter months, give a greater quantity of milk and butter, and of better flavour, than those fed upon potatoes. The butter is nearly equal to that from spring grass. Though the root of this plant has the quality of improving that article, it must be observed, that the leaves give it a very disagreeable taste, which, however, is of no consequence when intended to be potted, as it goes off in a short time.

Parsnips are dangerous food for sows before they farrow, and might occasion them to lose their litter. Hogs may be fattened with them in about six weeks. It is the custom during

during that time, to thicken their swill with the meal of beans and oats ground together. Pork fattened in this way is very firm, and does not waste in boiling.

Horned cattle may be fattened with parsnips in about three mouths. I never knew them used for sheep.

It is the general opinion in the island, that hogs or cattle fed on parsnips may be brought in a condition for slaughtering in less time, and with half the quantity that would be required of potatoes. The butchers are sensible of the superiority of the former, and will give a halfpenny per pound more for cattle fattened with them, than for such as have been fed any other way. Upon inquiry I was informed, they always contained a greater quantity of tallow.

This I believe to be a full account of the culture and use of the parsnip, and a just comparison with the potatoe. Should the Society wish any further information, either on this, or on my Telegraph, I shall think myself in duty bound to give it.

I remain, sir,

Your most obedient humble servant,

CHARLES LE HARDY.

The above communication was accompanied with certificates of the correctness of the statements which it contains.

XXXI. *Analysis of the Carbonated Chalybeate Well, lately discovered at Middleton Hall, the Seat of Sir* WILLIAM PAXTON, *Kt., near Llanarthney in Carmarthenshire. Communicated by Mr.* HOWELL. *The Analytical Results from an Analysis of Mr.* ACCUM.

MEDICINAL waters have from time immemorial been much resorted to by the afflicted; and many traditionary as well as written accounts of their virtues have been transmitted from one age to another.

During the first efforts of science, accident seems to have given some of these waters an illegitimate value, and prejudice or fiction clothed others with mystery.

Before the Christian æra, the effects of particular waters were known; and to some, such as the fabled waters of Lethe, supernatural powers were attributed. Soon after the commencement of that æra, medicinal springs increased every where, and superstition found in almost every situation a holy or canonized well. The first rays of reformation exposed the impotency of many, and the unchastened glare that too often leads into extremes, brought others de-

 servedly

servedly into contempt. Those individuals, however, who had received benefit from particular springs, could not forget their obligations; and others being daily relieved, a series of evidence presented itself that could not be resisted; and therefore a few springs retained their celebrity, whilst others fell into deserved neglect.

The medicinal spring under consideration has been lately discovered in the park of Sir William Paxton, at Middleton Hall, near Llanarthney. The medicinal effect which this spring has already produced, bids fair to hope that this water will occupy a very distinguished place among those fountains of health which can never be viewed with indifference. The water of this spring has been analysed by Mr. Accum of London. Omitting to state the analytical processes by which this philosopher renders his results legitimate, it will be sufficient to announce the summary of the analysis, which is as follows:

Gazeous Contents in 100 *Parts.*

	cubic inches.
Carbonic acid gas - - - - - -	16·50
Atmospheric air - - - - - - -	4·50
Cubic inches	21·

Solid Contents in 100 *Parts.*

	grains.
Carbonate of iron - - - - - -	5·25
Muriate of soda - - - - - -	6·00
Carbonate of lime - - - - - -	4·75
Muriate of lime - - - - - -	3·25
Sulphate of lime - - - - - -	2·00
Grains	21·25

XXXII. *On the Preparation of a Fibrous Substance from Bean Stalks, applicable to the Uses for which Hemp is employed. By the Rev.* James Hall, *of Walthamstow**.

Sir, Though it has not been attended to, nor, so far as I know, has ever been mentioned by any one, yet it is certain that, according to its size, every bean plant contains from 20 to 35 filaments, or fibres, running up on the outside, under a thin membrane, from the root to the very top

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1809.——The Society voted their silver medal to Mr. Hall for this communication.

all

all around, the one at each of the four corners being *rather thicker* and stronger than the rest. It is also certain that, next to Chinese, or sea-grass, in other words, the material with which hooks are sometimes fixed to the end of fishing lines, the filaments or hempen particles of the bean plant are among the strongest yet discovered. These with a little beating, rubbing and shaking, are easily separated from the strawy part, when the plant has been steeped 10 or 12 days in water; or is damp, and in a state approaching to fermentation, or what is commonly called rotting. Washing and pulling it through hackles, or iron combs, first coarse and then finer, is necessary to the dressing of bean hemp; and, so far as I have yet discovered, the easiest way of separating the filaments from the thin membrane that surrounds them.

From carefully observing the medium number of bean-plants in a square yard, in a variety of fields on both sides the Tweed, as well as in Ireland, and multiplying them by 4840, the number of square yards in an acre, and then weighing the hemp or filaments of a certain number of these stalks, I find that there are at a medium about 2cwt. of hemp, or these filaments, in every acre, admirably calculated for being converted into a thousand articles, where strength and durability is of importance, as well as, with a little preparation, into paper of all kinds; even that of the most delicate texture.

Now since there are at least 200,000 acres of ticks, horse and other beans, planted in Great Britain and Ireland, and since, where there is not machinery for the purpose, the poor, both young and old, females as well as males, belonging to each of the 9700 parishes in England, &c. where beans are raised, might (hemp having risen of late from 60 to 120 pounds per ton) be advantageously employed in peeling, or otherwise separating these filaments from the strawy part of the plant, after the beans have been thrashed out; I leave it to the feelings of the Society for the Encouragement of Arts, &c. to judge of the importance of the idea held out here, not only to the poor, but to the land-holders and the community at large.

It is nearly twelve months since, by analysing its component parts, I discovered hemp in the bean plant. I would have written to you then, Sir, on the subject, and sent a specimen, but that I was trying experiments with other plants, as I am during my leisure hours doing at present; and I wished to ascertain in what degree this species of hemp is liable to injury from different situations, and the changes of the atmosphere. With a view to this, I exposed

 one

one parcel nearly 12 months, to all the varieties of the air within doors, and kept another nearly as long *constantly* under water, and find them not in the least injured. The chief difference I perceive is, that the one kept constantly under water, namely the *whitest* of the specimens sent you, has assumed a rich silky gloss, and a much more agreeable colour than it had before.

But though this is the case with bean-hemp *after* it is cleaned and dressed, and which, though stiff and hard when dry, is pliable and easily managed when rather damp or wet, it seems otherwise with it *previous* to its being separated from the straw. If bean-straw be kept for years under water, or quite dry, it produces, I find, hemp as good and fresh as at first. But if the straw be sometimes wet and sometimes dry, the filaments or fibres are apt to be injured. The specimens of bean-hemp accompanying this letter, in the form of oakum for caulking ships, having been long exposed to the varieties of the weather, previous to being separated from the straw, is a proof of its being considerably injured. If the straw of the bean was scattered thin on the ground, and exposed to the weather for two or three months, I have uniformly found that the hemp, or fibres, are loosened, and easily separated from the strawy part, without any other process than *merely* beating, rubbing and shaking them, and perhaps this is the easiest way of obtaining bean hemp; but then, from being thus exposed, and the fermentation that takes place in the strawy part, which is of a spongy nature, communicating itself to the fibres, or hemp, I find that these are generally less or more injured, though not so much so, in my opinion, as to prevent them from being excellent materials for making paper.

I have also found, and the importance of the idea will, I hope, be an excuse for mentioning it here, that, though the water in which bean straw has been put to steep, in a few days generally acquires a black colour, a blue scum and a peculiar taste, yet cattle drink it greedily, and seemed fattened by it. But my experiments have hitherto been on too limited a scale to be able, in a satisfactory manner, to ascertain this last circumstance. When the water in which bean straw has been put to steep, becomes fœtid, which I find it is *scarcely* more apt to become than common stagnant water, on being stirred by driving horses or cattle through it, by a stick, or in any other way set in motion, (as is the case with all putrid water, even the ocean itself,) the fœtid particles fly off, and the effluvia dies away.

When straw is to be steeped for bean hemp, the beans are

are to be thrashed in a mill: the beans should be put to the mill, not at *right angles*, but on a *parallel*, or nearly so, with the rollers, else the straw, particularly if the beans are very dry, is apt to be much cut. If the straw is *not* to be steeped, on putting the beans to be thrashed at right angles, or nearly so, with the rollers of the mill, a certain proportion of the fibres, or hemp, may easily be got from the straw, these being in general not so much cut as the straw; but often found torn off and hanging about it like fine sewing threads. The hemp thus taken off, though its lying under water for months would do it no harm, requires only to be steeped a few minutes, drawn through a hackle and washed, previous to its being laid up for use. If the hemp, or fibres, collected in this way (which is a fine light business for children, and such as are not able for hard work, and which requires no ingenuity), are intended only for making paper, they require neither steeping nor hacklings, but only to be put up into parcels and kept dry till sent off to the manufacturer.

The straw of beans contains a saccharine juice, and is highly nutritive, perhaps more so than any other; and, like clover, the prunings of the vine, the loppings of the fig-tree, &c. produces a *rich* infusion, and commonly fine table-beer, as well as an *excellent* spirit by distillation. It is the hemp or fibres that prevents cattle from eating it. These, like hairs in human food, make cattle dislike it. The collecting of it, therefore, should never be neglected, nor the boys and girls in workhouses and other places be permitted to be idle, while business of this kind would evidently tend both to their own and their employers' advantage.

It is a fact, that about the generality of mills for beating and dressing hemp and flax, a large proportion, in some inland places both of Great Britain and Ireland, amounting nearly to one-half of what is carried thither, is either left there to rot, under the name of refuse, or thrown away as of no use, because too rough and short for being spun and converted into cloth. Now, from the experiments I have tried, and caused to be tried, I have uniformly found, that though too rough and short for being converted into cloth, even of the coarsest kind, the refuse of hemp and flax, on being beat and shaken, so as to separate the strawy from the stringy particles, which can be done in a few minutes by a mill or hand labour, as is most convenient, becomes thereby as soft and pliable, and as useful for making paper, as the longest, and what is reckoned the most valuable part of the

 plant,

plant, after it has been converted into cloth and worn for years.

In its natural state, it is true the refuse of hemp and flax is generally of a brown and somewhat dark colour. But what of that? By the application of muriatic acid, oil of vitriol, or other cheap ingredient, well known to the chemist, as well as to every bleacher, such refuse, without being *in the least* injured for making paper, can, in a few hours, if necessary, be made as white as the finest cambric.

There are, at a medium, published in London, every morning, 16,000 newspapers, and every evening about 14,000. Of those published every other day there are about 10,000. The Sunday newspapers amount to about 25,000, and there are *nearly* 20,000 other weekly papers, making in all the enormous sum of 245,000 per week. At a medium 20 newspapers are equal to one pound—hence the whole amount to about 3 tons per week, or 260 tons per annum. But though this, perhaps, is not one-half of the paper expended in London on periodical publications, and what may be called fugacious literature, and not one-fourth part of what is otherwise consumed in printing-houses in the country at large, yet there are materials enough in the refuse of the hemp and flax raised in Britain and Ireland for all this and much more.

Nor is this all: for as the bine or straw of hops, a circumstance well known to the Society, contains an excellent hemp for making many articles, so also will it prove a most excellent material for making all kinds of paper. And it is a fact, that were even the one-half of the bine of hops raised in the counties of Kent Sussex, and Worcester, instead of being thrown away, or burnt, after the hops are picked, as is commonly done, steeped for ten or twelve days in water, and beat in the same way as is done with hemp and flax, independent of what might be got from bean-hemp, and a variety of articles well-known to the Society, there would be found annually materials enough for three times the quantity of paper used in the British dominions.

I have the honour to be,
with much respect,
Sir,
Your most humble servant,
JAMES HALL.

Streatham, Jan. 9, 1809.

To C. TAYLOR, M. D. SEC.

Certificates

Certificates of the Truth of the foregoing Statement.

Streatham, Surry, Jan. 9, 1809.

We, the undersigned, do hereby certify, that the specimens of hemp inclosed and sealed up by us, addressed to Dr. Taylor, secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce, Adelphi, Strand, are the produce of common bean straw :—That we never saw nor heard of bean hemp till lately ; when the Rev. James Hall, who resides here at present, was trying experiments respecting it at Mr. Adams's farm, Mount Nod, and other parts of this parish :—That, in the present obstructed state of commerce with the continent, it appears to us the discovery of bean hemp may be extremely useful to the manufacture of canvass, ropes, paper, &c. ;--and that, as it affords a new and important prospect of employment for the poor, we think Mr. Hall, the discoverer, is deserving of the approbation of the public. We shall only add, that as the Society for the Encouragement of Arts, Manufactures, and Commerce, have contributed so often in a high degree to the exertion of genius, the improvement of the arts, and the public good, we have no doubt but they will not only take the proper steps to prosecute the discovery and encourage the manufacture of bean hemp, but also, by some mark of their favour, show their approbation of Mr. Hall's merit in the discovery he has made, as well as of his high public spirit and liberality in communicating the discovery to the public without reserve.

William Adams, Mount Nod.
Edward Bullock, Curate.
Wm. Gardner, Surgeon.

Streatham, Surry, Jan. 9, 1809.

These are to certify to the Secretary of the Society for the Encouragement of Arts, &c. London, and all whom it may concern, that having seen (at first to our astonishment) the Rev. James Hall, who has resided here for some time past, procuring hemp from common bean straw, steeped some days in water, we steeped some also, and easily got hemp from it ; there being no mystery in the matter more than *merely* steeping the straw, peeling off the hemp, and then washing and cleaning it, by pulling it through a hackle or comb.

These are also to certify, that having tried bean hemp, and

and found it to take both wax and rosin, we have sewed with it, and find the fibres of which it consists in general so strong, that the leather never failed to give way sooner than the seam. We have only to add, that as hemp has of late become uncommonly dear, while much of it is bad, we anxiously wish the prosecution of the discovery, and the appearance of bean hemp in the market; and shall, so soon as we hear of its being spun and on sale, be among the first to purchase and use it.

John Houne, Shoemaker.
Thomas Alford, Shoemaker.

Letter from Mr. Hume, of Long Acre, to the Rev. James Hall.

Sir, I inclose a specimen of the bean filaments or thread which have been submitted to the bleaching process. The texture and strength seem not in the least to have been impaired, but retain the primitive tenacity; and I am persuaded this substance will prove an excellent substitute for hemp and flax, for the manufacture of various kinds of paper, cordage, and other materials. I did not find more difficulty in accomplishing the bleaching of this than in other vegetables which I have occasionally tried, and I believe this article is susceptible of a still greater degree of whiteness.

I remain, sir,
Your very obedient servant,
Jos. Hume.

Long Acre, Feb. 24, 1807.

Letter from Mr. H. Davy to the Rev. James Hall.

Sir, I shall inclose in this paper a small quantity of the bean fibres, rendered as white as possible by chemical means. It seems to bear bleaching very well; and, as to chemical properties, differs very little from hemp.

The question, Whether it is likely to be of useful application, is a *mechanical* one, and must be solved by experiments on its comparative strength. I am, sir,

Your obedient humble servant,
H. Davy.

XXXIII. *On*

XXXIII. *On Crystallography. By M.* Haüy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 153.]

OF MINERALOGICAL METHODS.

All the productions of nature, considered in the point of view in which she presents them directly to our eyes, form a picture complicated with a multitude of details, in the midst of which the eye is lost at the first glance, and sees every thing at once without distinguishing any thing.

With the view of facilitating the study of this picture, there have been contrived, with respect to mineralogy, as with zoölogy and botany, methodical distributions of the subjects which are therein embraced; their different parts have been dissected in imagination, so as to form a kind of factitious table, with which we may afterwards compare the former, and which serves it as a kind of explanation.

However slightly we reflect on the progress of these methodical arrangements, we may easily perceive that they are founded on the faculty possessed by the human mind of regarding certain qualities in an object, by abstracting others; and of raising ourselves gradually from particular to general ideas.

Thus, when speaking of an oak as a determinate object which I can point out with my finger, I make no abstraction; I consider in the object which I name, all the qualities that can accord with it: in a word, I designate an individual, *i. e.*, a being which has a particular existence. But if, in pronouncing the word OAK, I have not seen any particular oak, then I abstract the idea of a particular existence; I designate in general a collection of individuals similar in all their parts, and this collection is what is called a SPECIES.

The sense in which I have taken the word Oak (or *Quercus*) is that which every body attaches to it in ordinary language. Now, on comparing the individuals of the species in question with those of another species, to which the name of Holm Oak (*Ilex*) has been given, I remark that the latter have the organs of the flower similarly constructed, and that their fruits are acorns also; but that they differ from each other in several respects, and particularly in the form and consistence of the leaves; which in the former are broad, soft, and terminated by round lobes, and in the latter narrow and indented at the edges. I can, therefore, fix my attention solely on the resemblance of the flower

flower and the fruit in the individuals of the two species, keeping separate in my imagination all the parts which differ; and in order to adapt the nomenclature to this resemblance, which alone occupies my mind, I shall extend the name of oak to both the species. Referring my mind afterwards to the differences which I have left on one side, I shall keep an account of them in language, distinguishing by the name of *common oak* the individuals of the first species, and by the name of *green oak* those of the second. I shall then have a genus, of which the common oak and the green oak will be two species.

By a new abstraction I can consider in the two oaks nothing but their size, ligneous consistence, and the faculty which they have of existing a certain number of years; and observing that several kinds of productions, different from oaks, have also a great consistency and are very long lived, while a multitude of other species are of lower stature, more pliant, and exist a year or two only, I shall unite, under one and the same idea, the first by the name of *trees*, and I shall designate in common all the others by the name of *shrubs*. I shall thus have two great classes*, each of which may be subdivided into a certain number of genera, which will be groups of species. Finally, if I have no longer any regard but to the faculty which all these objects have of vegetating, and of being nourished from the juices of the earth, I shall include them under the general denomination *plant*, and I shall thus attain, by a series of ideas always more abstract, the most elevated point of view of the vegetable kingdom.

Human languages present a host of examples of similar abstractions, which a natural spirit of analysis has suggested even to the vulgar; and it is by directing their views in the same manner that the learned have formed their systems and methods. They have merely subjected these methodical arrangements to more precise and more rational principles; they have multiplied their divisions and subdivisions, and have in some measure arranged them by the indication of the characters peculiar to the objects which each division contains.

We see from what precedes, that in proportion as we ascend into the course of abstractions, we connect together a greater number of beings, according to the relation or character analogous to the degree of abstraction. Thus the

* I do not pretend to establish rigorous limits here between the divisions of bodies, but merely to give a sketch of the progress of ideas by examples taken from familiar objects.

idea

idea which the word *tree* expresses, embraces incomparably more plants than that which is attached to the word *oak*, and the latter has a greater latitude than the idea presented to the mind by the word *green oak*. Reciprocally, every abstraction from an inferior degree compresses into a smaller space the number of the objects to which it is extended. What does methodizing effect then?—It divides and subdivides successively the assemblage of objects, according to their various characters or relations, so that, at every division, all the characters enunciated in the preceding divisions being regarded as still subsisting, the method adds the expression of a new character, a new trait of resemblance, which detaches the objects contained in this division. The more the sum of the relations increases, and the more on the contrary the number of objects with which these relations agree are diminished, and when this sum is the greatest possible, when it is extended to all the faces of the objects which it includes, each of these objects is considered as representing all the others, and we say that all these objects are of the same species.

On the other hand, in proportion as the degree of abstraction is raised, the number of subdivisions which answers to this degree diminishes: and it was this manner of regarding methodical order that the illustrious Bacon had in view, when he compared Nature to a pyramid the base of which was occupied by an almost infinite number of individuals; above this base rise the species formed by the assemblage of individuals, and which are consequently extended over a narrower space than the base; afterwards come successively the genera composed of species, then other superior genera (which answers to our orders and classes); until Nature, after having become narrower and narrower, terminates in a point, or in unity*.

We may also be able to see that the character which served to connect with each other the productions of one and the same division, distinguished them from those of another division. Hence, and from all that precedes, result two remarkable advantages of the method. The first is, to make us acquainted with objects not only by themselves but also by comparison, each of them being placed by the method in such a manner that it turns in some measure towards the rest the side in which it resembles them, and presents in an opposite direction that by which it is distin-

* Bacon, *De Augment. Scient.* t. ii. c. 13. See the work which has for its title "Le Christianisme de François Bacon." Paris, an 7, t. i. p. 1.

guished

guished from them. The second advantage is, that after we have been exercised in making applications from the method to a certain number of objects already known, we may attain a knowledge even of that which would be new to us, by consulting successively the characters which accompany each division, and by making use of the method to inquire into the subject, and to learn from the object itself the place which it occupies in the method.

The series of divisions and subdivisions in mineralogical distributions is nearly the same as in those which regard organic bodies. This series, taken by descending from generals to particulars, gives the following gradation: classes, orders, genera, species, varieties. But there is a sensible difference relative to the methods used in these two departments of science, relative to the manner in which we consider objects, or to the choice of the methods employed to classify and characterize these objects.

Thus, in botany, we call *species* the succession of plants which reproduce each other. In mineralogy, there is neither reproduction nor species, if we take this term in a rigorous acceptation. There is nothing, however, to hinder us from following the example of Linnæus, Bergman, and several other celebrated naturalists, in applying the word *species* in a wider sense, to an assemblage of inorganic beings which have a common basis, and the differences of which ought to be regarded as purely accidental.

But this leads us to an important question, to which it does not seem that sufficient attention has hitherto been paid. In what consists in the present case the type of the species;—and when are we justified in regarding several minerals as belonging to one and the same species*? It seems at first view as if chemical composition was the basis of this union; so that the true notion of the species consists in conceiving an assemblage of minerals formed of the same principles united to each other according to the same laws. But we shall see how much this idea is susceptible of restriction, and to what point even we should wander from our object, in a multitude of circumstances, on taking it for our guide, in assembling of varieties which ought to bear one and the same specific name.

* I shall by and by recur to the reasons which induced me to apply this word, rather than that of *genus*, to the different objects which in the language received among naturalists have a common name, such as *topaz*, *emerald*, *garnet*, &c., or, if an *acidiferous* substance is alluded to, the term *carbonated lime*, *sulphated barytes*, &c. It is sufficient for my purpose at present to point out such of the divisions and subdivisions of the method to which I give the denomination *species*.

In

In order that I may be better understood, I shall take an example from feldspar. Mr. Kirwan, to whom we are indebted for a treatise on mineralogy, in which that celebrated author has brought together, in the development of the science, the external characters of minerals and the results of his own researches as well as of those of other chemists, as to the composition of these bodies, cites 13 analyses of the substance in question, to which we may add a 14th made by Vauquelin. Now, not only do the products vary among each other in the proportions of the same principles, but there are ingredients which are found in certain products, and not in others. Thus Mr. Kirwan has procured from a reddish feldspar eleven per cent. of barytes and eight of magnesia; whilst the result obtained by Wiegleb in another feldspar of a red colour, furnished neither of these earths, but only silex and alumine, with a small quantity of oxide of iron and fluoric acid. Vauquelin found about one seventh of potash in the feldspar called *adular*, and in the green feldspar of Siberia, and yet no other analysis has presented this alkali. Besides, this expert chemist has discovered neither magnesia nor barytes in the same mineral.

Mr. Kirwan concludes, from the different analyses quoted by him, that every compound of silex and alumine (the silex being predominant) to which is added a slight proportion of lime and magnesia, or of lime, of magnesia and barytes, (but sufficient to render the whole fusible at a degree of heat not exceeding 140°,) would form a feldspar; and we ought not to hesitate in giving it this name, if at the same time it presents a lamellous texture. But he adds that iron seems in this case to be an accidental principle.

I do not observe that this rule laid down by Mr. Kirwan leaves any thing to be desired, so far as simplicity and precision are concerned; but in spite of the efforts of the author to render it general, at the hazard of loading it with conditions, it is no longer applicable to the result of the analysis made by M. Vauquelin of the feldspar known by the name of *adulary*; and finally, if we should undertake to give similar rules for all minerals, the result would be a complication from which it would be difficult to extricate ourselves, and it would even very probably, happen, that a rule which should have for its object such a particular species might be applied nearly equally well to an entirely different species.

I shall not examine if all the analyses alluded to by Mr. Kirwan deserve an equal confidence. But we may at least conclude

conclude that they indicate perceptible differences of composition between the specimens analysed. I could produce other examples of a mineral, the different analyses of which made by skilful operators have given different products; and we shall presently see that this must necessarily take place in a variety of circumstances.

Now I return, and I ask, on what foundation Mr. Kirwan gives the name of feldspar to the various specimens which have been the subjects of the analyses above mentioned. This intimacy surely is not founded on the results of these analyses; since we should rather be inclined to infer, from the differences which they have presented, that a few at least of the substances to which they are referred, constitute distinct species. In a word, it is visible that Mr. Kirwan has tacitly supposed that, abstracting all consideration of the analyses, the substances in question had been regarded as feldspars.

On perusing what has been written by naturalists on the subject of this mineral, we see that its place was assigned according to a certain assemblage of characters, such as a hardness capable of producing sparks with steel; a leafy texture added to its breaking into rhomboidal fragments: a specific gravity of about 2·5; a fusibility into a white enamel; &c.

But these characters are for the most part variable to a certain point; and this variation may even be extended far enough, in certain cases, by a consequence of that which the component substances undergo. These are so many useful ways of assisting us to recognize bodies which belong to one and the same species; but in addition to their not presenting to the mind a sufficiently simple and precise idea of what constitutes this species, their results are not always proper for tracing the limit which separates one species from another: and it is on account of our being restricted to consulting them only, that we have confounded the pyroxene with the amphibolus, the chabasie with the mesotype, and so on with several other relative connexions, the fault of which will become evident from what we shall say under the respective heads of the substances to which they are referred.

There exists a character much more solid and much more proper by its invariability, to serve as a rallying point to the different bodies, which belong to one and the same species. This is derived from the exact form of the integrant molecule, because this form subsists, without any sensible alteration, independently of all the causes which may

may cause the other characters to vary. Thus, in order that we may not quit the example of feldspar, such in this substance is the arrangement of the natural joints, that the molecule resulting from it is an oblique-angled parallelopipedon, in which the three plane angles which concur to the formation of one and the same solid angle, form among them a first angle of 90°, a second of 120°, and a third of 111° and a half; and these angles will be constantly the same in specimens variously crystallized; in those which will give by analysis barytes or potash, as well as in those which will exhibit no vestige of it.

And not only may we estimate by observation combined with theory the angles of the integrant molecule, but we even succeed in ascertaining the relations between its dimensions, and there results a geometrical form perfectly determinate, which is the same in all the individuals of the species, and which presents as it were a fixed point in the midst of the oscillations of all the other characters; so that we may even say, that in general the bodies of each species touch closer in the results of the theory relative to their structure than in those of chemical analysis.

I do not pretend to raise the character I have just mentioned above its real importance. I am even led to suspect the predilection which I ought naturally to have for that character which belongs to a branch of mineralogy which I have cultivated with particular care. But this predilection does not prevent me from stating a truth which I think useful to the progress of science; which is, that this character, borrowed from the structure, ought to have a great influence in the distinction of the species; and by neglecting it we deprive ourselves of one of the most advantageous methods for the formation of an exact and regular system.

It may be objected, that the determination of the integrant molecule of a body is frequently a delicate operation, which requires minute experiments, and besides presupposes a knowledge of calculation which the whole world is not in possession of. But chemical analysis has also its difficulties, and is not the affair of a moment. It requires a good deal of art to employ the most proper agents for seizing and coercing principles invisible, and, if we may be allowed the expression, impatient to escape from the hands of the chemist: it requires art also that the operation may take nothing from the result that belongs to it, and add nothing that is foreign to it; and sometimes it is only by repeatedly recurring to the subject, that we succeed

 in

in forming the faithful picture of the relations which existed among these various principles combined with each other in the substance as yet untouched. We never purchase at too high a price what contributes to the perfection of a science; and we ought not to calculate time when it is necessary to arrive at immutable truths.

It will perhaps be said also, that a mineral substance is found in compact or granulous masses, which refuse to submit to any mechanical division. I shall answer, that frequently also these masses form a continuity with crystallized substances, or such as have a lamellated texture, in such a manner that it is visible that they are referred to the same species; and as to those which we meet with in an insulated state, if nothing more is then wanted to determine them, except characters less certain than that which is derived from the structure, it merely results that we ought to regret that this last character has not a greater generality; and this very regret is a kind of avowal of its preeminence, in every case in which it can be employed*.

Will it be said that there are forms of integrant molecules which are common to substances of different natures? I shall observe, in the first place, that this does not take place except with respect to solids which have a peculiar character of regularity, in such a manner that in all the other cases the form of the integrant molecule is sufficient of itself for determining the species. I shall answer afterwards, that most of the substances which have one common molecule (and this may be said of all those which, like the ductile metals, never have a lamellous texture) may be easily distinguished by other characters. For example, the cube agrees best as the integrant molecule with borated magnesia, muriated soda, sulphurated lead, sulphurated iron, &c.; all of them ascertainable independently of mechanical division.

In a word; all that I wish to infer from this discussion is, that the character drawn from the structure ought to occupy a very distinguished rank among those which are made use of to mark the general character (*triage*) of

* Sometimes the rareness of crystals is alleged as a proof of the scanty resources furnished by the character drawn from crystallization. This difficulty does not seem to be well founded, since a single crystal clearly defined, is sufficient to determine a multitude of irregular masses, which, with this crystal, would have relations indicative of an identity in nature: and if, by objecting that crystals are rare, it is meant to say that there are several mineralogical species which are never presented under crystalline forms, I shall ask if these are properly species *per se*, and not on the other hand mixed species, in the production of which different species have concurred?

original

original bodies of one and the same species. It certainly has its obscure sides, and there are circumstances in which it disappears. But wherever it shows itself, there is a ray of light against which we ought not to shut our eyes.

I shall add, that, with a little expertness in applying calculation to theory, we may decide if a given form enters into, or ought to be excluded from, any given species. Thus we shall find that the cube which has been quoted among the varieties of carbonated lime, is foreign to the crystallization of this substance*. Now it is easy to see of what service this adaptation of calculation must be, in order to separate the crystallized minerals into their respective species, assigning to each what belongs to it, and freeing it from what it might have usurped.

All that precedes leads us to an interesting consideration relative to the chemical composition of minerals: viz. that the principles which concur to form their integrant molecules must, as I think, be constant, both as to their quantities and their qualities, in such a manner that the substances which cause a variation in the products of the analysis are foreign to the molecules, and merely interposed among them in the mass of the mineral†. We may compare a substance mixed with these additional principles to certain salts with which other salts are accidentally united, as is the case with nitre of the first formation. When we make this salt undergo successive solutions and crystallizations in order to purify it, the liquid in no respect alters the figure of its molecules; it only separates them from each other, and frees them from those of the other salts which were associated with them, and which had gone for nothing in their composition. In the same way the principles on which the differences between the analyses of various pieces of one and the same mineral depend, merely form with the substance peculiar to the latter a simple mixture, from which the integrant molecules would come out untouched, if power were not given to us to regulate, if we may be allowed the expression, their departure‡.

From

* Vide under the head Carbonated Lime, the variety which we have called *cuboid.*

† I even think that, in the case in which we say that there is an excess of one of the principles in other respects essential to the composition of a mineral, the superabundant part goes for nothing in the formation of the molecule, and ought to be ranked among heterogeneous and purely accidental principles.

‡ These are the accidental principles which produce a variation in certain external characters, such as colour, lustre of external surface, of fracture,

From these considerations it appears to me that we may define a species in mineralogy to be, *A collection of bodies, the integrant molecules of which are similar, and composed of the same elements united in the same proportion.* This last condition generalizes the definition, and extends it to substances which, having their molecules of the same configuration, differ essentially in the principles which compose these molecules.

Some mineralogists are of opinion that these collections which I have called species ought rather to be regarded as genera. But where then would be the species which would subdivide the genus? Would these be crystals of different forms? It seems to me that these modifications, which in truth are the results of so many determinate laws, but which after all belong only to local circumstances, such as the density or other qualities of the liquid,—do not furnish a sufficient reason for establishing specific distinctions between them. They do not touch the substance, and are confined to the giving of different envelopes to one and the same nucleus. Besides, on the supposition in question we should be embarrassed with unshapen masses, which surely do not deserve to be erected into species. The same answer applies to the hypothesis on which the species would form groups, one of which, for example, would comprehend bodies regularly crystallized; a second, concretions, &c. But let us conceive that the molecules which produced the concretion had been freely suspended in a tranquil liquid; they would have assumed another arrangement, and might have formed crystals. The idea originated by the word *species* goes straight down to the bottom of the substance, and does not stop at simple windings.

We should not be better founded in regarding as so many species the mixtures of a substance with accidental principles, which only modify the principal species, but do not transform it into another which may be really distinguished from it. Even these mixed bodies only belong to the

ture, &c. Thus we find at mount Vesuvius crystals of pyroxene (Augit of Werner), the surface and fracture of which have a very shining appearance, whereas amongst those of Norway several have a rough and dull surface, and their interior is not very shining. Nevertheless both are divided under the same angles, and have forms either similar, or which may be referred to the same molecule; so that the species to which they belong preserves its unity, in spite of the differences of aspect which we have mentioned; and consequently these differences are all of them at most the index of a variation in the principles which concur in any given way to the composition of the substance, but they do not announce any variation in the essential and truly constituent principles.

principal

principal species, because they admit of the existence, at least in part, of the predominant characters. If this was not the case, they ought no longer to occupy a place in the method: they ought to be thrown into the general appendix, in which are placed the mixed substances called *rocks*. And this shows how contrary it is to the spirit of the method, to place on the same line (as so many particular species) with the true species, marles, argils, schists, and other bodies, which are only fortuitous aggregates of species already classed in other places in the method, and no one of which imprints its character on the whole; so that we are even unable to decide to what species they ought to be referred, as being but a simple appendage.

From all that I have said, we shall easily conceive how important it would be to determine by the aid of analysis, (with respect to each species) those principles which concur by themselves to the formation of the integrant molecule, by operating on picked pieces, the composition of which contains only what cannot be dispensed with, without ceasing to be what it really is, and which had borrowed nothing, if we may be allowed the expression, from the liquid in which it had originated. We should thus have the limit from which the analyses of other pieces remove more or less, according as the latter contain principles purely accidental, or if one of the constituent principles is found there in excess. This limit would give what would be called the analysis of the mineral submitted to experiment, and the other results would make known the accidental diversities of which the composition is susceptible: they would serve to indicate to what term a certain principle has varied in its proportions, and to unveil the principles which have only a transitory existence, and are rather a surcharge with respect to the mineral which contains them, as they do not contribute to its integrity.

I think I have sufficiently shown how much strength may be acquired by chemistry and mineralogy by their mutual cooperation. Without the former we should be ignorant in what class a mineral ought to be placed if it contains an acid, or if earths only enter its composition, or if it does not conceal a metallic substance under the appearance of a simple stone. Without the latter art, it would frequently be difficult to refer to the type of the species the varieties which appertain to it. The one indicates the first link of the chain, and marks the point to which it ought to be attached; but the intervention of the

other is necessary for continuing this chain and arranging the different links of it.

I trust that this long discussion will be forgiven. I thought it necessary, because it appeared to me that at no time has the influence been sufficiently acknowledged, which mineralogy ought to have on a well-arranged method; and because, if there are cases in which the mineralogist cannot refrain from saying to the chemist, *Make me acquainted with the substance which you have analysed,*—there are others in which the chemist, if he is cautious, ought to say to the mineralogist, *Make me acquainted with the substance which I have analysed.*

M. Vauquelin, who adds to dexterity of operating a great accuracy of reasoning, has proved more than once, that he did not regard the consequences inferred from the geometry of crystals as useless in assisting us to fix the term at which the analysis ought to begin. Placed by circumstances beside each other, we have frequently consulted together on the subject of our inquiries, and the results which we attained, by two methods of interrogating nature so different, were mutually of service as guarantees by their conformity. I feel strongly the advantages which this cooperation has produced, and I am anxious that it should be publicly known, that it is in the *Ecole des Mines* in France that chemistry and crystallography, so long separated, have contracted a strict alliance, which bids fair to be of long duration.

If we now resume the comparison between the methods of botanists and those of mineralogists, we may observe that the former are entirely founded on characters furnished by observation of the external form, but the latter having a necessary connexion with the internal organization, which is constant in all the individuals of one and the same species, each of them may serve as a common model, in order to paint as if at a single stroke the entire species.

In the mineral kingdom, on the contrary, in which the external characters undergo continual variations, in which even the best defined forms are only evanescent disguises, nothing that speaks to the eye can serve as a foundation to the method. It belongs to analysis to lay this foundation, and to regulate the order of classification, making use on all occasions of the light afforded by crystallography. But this object once attained, the observer must be able to ascertain the substances classified by methods independent of

of analysis, by employing a choice of characters happily combined, some of which are presented as of themselves to our senses, and the others only require, for being regularly defined, prompt and easy operations.

Here crystallography, which had seconded analysis in the formation of the method, will reappear to advantage, in order to furnish characters founded on the angles of crystals, which may be measured in an instant.

Thus the means which have presided in the composition of the method will be at the same time the most solid, and those which will be the least arbitrary, and the means which shall direct the use of the method will have the merit of simplicity and convenience.

It may be seen from what precedes, that a mineralogical system, being complicated with characters frequently borrowed from very different considerations, requires more cautious proceeding in the person who employs it than a botanical system, the uniform and regular progress of which is traced according to a modification which speaks to the eyes, in such a manner that all the labour of the observer is confined to the different applications of one and the same principle. But this inconvenience, if it be one, is compensated in a great measure by the advantage of only having to determine among a number of species incomparably less than that which botany embraces: and the method being in this respect confined in a narrower circle, the observer finds his way more easily in the midst of the circuits which he is obliged to take in order to attain his object. We may add, that even the variety of the characters which he associates in one and the same inquiry, and the various kinds of knowledge which he combines with the observations of what is presented to his senses, contribute to give additional interest to the study of the objects which interest his mind, and to render this study at once more gratifying and instructive.

We have given in the preliminary discourse a detailed explanation of the method which we have adopted. The plan of it has been conceived in such a manner that, without suffering ourselves to fabricate connexions disavowed by nature, with a view to anticipate the results of analysis, we might profit by all those which should admit of our establishing a regular distribution of those parts of the method, in which the labours of the chemists have procured more certain and better connected information. We have divided the whole mineral kingdom into four classes, the titles of which we here place before the reader.

 1. Acidi-

1. Acidiferous substances; composed of an acid united to an earth or an alkali, and sometimes to both.

2. Earthy substances; into the composition of which earths only enter, united sometimes with an alkali.

3. Combustible substances (not metallic).

4. Metallic substances.

We have also explained the reasons which induced us to choose the base rather than the acid for characterizing the genera of the first class, and those to which metallic substances belong, which have also an acid for one of their component principles. Hence, in the last place, the necessity of modifying, by the help of a very simple inversion, the specific names adopted by modern chemists, when they say *sulphated lime* instead of *sulphate of lime*; *carburetted iron*, instead of *carburet of iron*, &c., in order to modify the nomenclature to the system itself, and to preserve that precision and regularity so happily introduced by Linnæus into the language of natural history.

The care which we have taken to restrict our method to what is founded on exact and precise experience, dictated that we should exclude certain substances, the nature of which is not yet sufficiently verified to admit of our deciding whether they constitute distinct species, or if they may be referred to any of the species already classified. We shall arrange these substances in a first appendix, in which we shall give the description of them, detailing the surmises which their characters may have already originated, as to what they would become if better known. A second appendix will contain the substances which are only admixtures of different species, and among which are found the aggregates known by the name of rocks, and which we shall therein more particularly describe. Lastly; there will be a third appendix for the productions of volcanos, and for those of subterranean fires, but not volcanic.

After all it must be remembered, that our methods, even when best managed, represent nature but imperfectly; and after having conducted us a certain length they leave us to ourselves. It should seem as if they had only been composed from selected pieces, in which the characters which they indicated had a clearer or better pronounced expression. Incomplete as they are, however, they have the precious advantage of putting our ideas in order, and prepare us for a more detailed study of their object. The instruction having been once begun by the exact application of the principles to selected specimens, is more easily and more happily terminated, by the close observation of

every

every thing which may be presented in nature; and the art of advantageously applying the method, leads an expert eye to find the art of knowing how to dispense with it altogether.

[To be continued.]

XXXIV. *On the Identity of Columbium and Tantalum. By* William Hyde Wollaston, *M.D. Sec.R.S.**

Within a short time after the discovery of columbium by Mr. Hatchett in 1801†, a metallic substance was also discovered in Sweden by M. Ekeberg‡, differing from every metal then known to him; and accordingly he described the properties by which it might be distinguished from those which it most nearly resembled. But although the Swedish metal has retained the name of Tantalum given to it by M. Ekeberg, a reasonable degree of doubt has been entertained by chemists, whether these two authors had not in fact described the same substances; and it has been regretted that the discoverers themselves, who would have been most able to remove the uncertainty, had not had opportunities of comparing their respective minerals, or the products of their analyses.

As I have lately obtained small specimens of the two Swedish minerals tantalite and yttro tantalite, from which I could obtain tantalum, and was very desirous of comparing its properties with those of columbium, Mr. Hatchett very obligingly furnished me with some oxide of the latter, which remained in his possession.

The resemblance was such in my first trials, as to induce me to endeavour to procure a further supply of columbium; and by application to the trustees of the British Museum, I was allowed to detach a few grains from the original specimen analysed by Mr. Hatchett.

Notwithstanding the quantity employed in my analyses was thus limited, I have, nevertheless, by proportionate œconomy of the materials, been enabled to render my experiments sufficiently numerous, and have found so many points of agreement in the modes by which each of these bodies can or cannot be dissolved or precipitated, as to prove very satisfactorily that these American and Swedish specimens in fact contain the same metal: and since the

* From Phil. Trans. 1809, Part II.

† Phil. Trans. for 1802.

‡ *Vetenkaps Academiens Handlingar.* 1802, p. 68.—*Journal des Mines*, vol. xii. p. 245.

re-agents

re-agents I have employed are in the hands of every chemist, the properties which I shall enumerate are such as will be most useful in the practical examination of any other minerals in which this metal may be found to occur.

In appearance the columbite is so like tantalite, that it is extremely difficult to discern a difference that can be relied upon. The external surface, as well as the colour and lustre of the fracture, are precisely the same; but columbite breaks rather more easily by a blow, and the fracture of it is less uniform, appearing in some parts irregularly shattered; nevertheless, when the two are rubbed against each other, the hardness appears to be the same, and the colour of the scratch has the same tint of very dark brown.

By analysis also, these bodies are found to consist of the same three ingredients; a white oxide, combined with iron and manganese.

Either of these minerals, when reduced to powder, is very readily acted upon by potash; but as the iron contained in them is not affected by alkalies, it appeared better to add a small proportion of borax.

Five grains of columbite being mixed with 25 grains of carbonate of potash and ten grains of borax, were fused together for a few minutes, and found to be perfectly incorporated. The colour was of a deep green, from the quantity of manganese present. The mass when cold could be softened with water, and a portion of the oxide could be so dissolved; but it seemed preferable to employ dilute muriatic acid, which, by dissolving all other ingredients excepting columbium, left the oxide nearly white, by the removal of iron and manganese that had been combined with it.

The muriatic solution having been poured off and neutralized with carbonate of ammonia, the iron was then separated by succinate of ammonia; after which the manganese was precipitated by prussiate of potash.

The products thus obtained from five grains of columbite, after each had been heated to redness, were nearly,

White oxide	4 grains
Oxide of iron	$\frac{3}{4}$
Oxide of manganese ...	$\frac{1}{4}$;

but it cannot be supposed that *proportions* deduced from experiments made on so small a scale can be entirely depended upon, although the *properties* of bodies may be so discerned, nearly as well as when larger quantities are employed.

An

An equal weight of tantalite taken from a specimen, of which the specific gravity of 7·8, yielded, by the same treatment,

White oxide	....	$4\frac{1}{4}$ grains.
Oxide of iron	....	$\frac{1}{2}$
Oxide manganese	......	$\frac{2}{10}$.

The white oxides obtained from each of these minerals are remarkable for their insolubility in the three common mineral acids, as both Mr. Hatchett and M. Ekeberg have observed.

In muriatic acid they cannot be said to be absolutely insoluble; but they are not sufficiently soluble for the purposes of analysis.

In nitric acid they are also nearly, if not perfectly, insoluble.

In sulphuric acid, when concentrated and boiling, the oxide of columbium may be dissolved in small quantity, and so also may the oxide obtained from tantalite.

The proper solvent, as has been observed by Mr. Hatchett and by M. Ekeberg, is potash; and as it is not required to be in its caustic state, I employed the crystallized carbonate of potash on account of its purity and uniformity. Of this salt about eight grains seemed requisite to be fused with one of the oxide obtained from either of these minerals to render it soluble in water.

Soda also combines with the oxide, and may be said to dissolve it; but a far larger proportion of this alkali is necessary, and a larger quantity of water. And although a solution may have been effected that is transparent while hot, it very soon becomes opaque in cooling, and finally almost the whole of the oxide subsides combined with a portion of the soda in a state nearly insoluble.

When a solution of the white oxide, obtained from either of these minerals, has been made, as above, with potash, the whole may be precipitated by the addition of an acid, and will not be redissolved by an excess of sulphuric acid, of nitric, of muriatic, succinic, or acetic acids.

But there is a further agreement in the properties of these two minerals, which appears above all others to establish their identity; for though they are both so nearly insoluble by any excess of the mineral acids, yet they are each completely dissolved by oxalic acid, by tartaric acid, or by citric acid; and the solution of each is subject to the same limitations; for if the precipitate has been dried, it is become intractable, and can scarcely be dissolved again till after a second fusion with potash.

If to the alkaline solution of either of them there be added infusion of galls, prussiate of potash, or hydrosulphuret of potash, no precipitate occurs; but when a sufficient quantity of acid has been added to neutralize the redundant alkali, the infusion of galls will then occasion an orange precipitate; but prussiate of potash causes no precipitate, nor does the hydrosulphuret precipitate the oxide, although the solution may become turbid from precipitation of sulphur by a redundant acid.

The characteristic precipitant of columbium is consequently the infusion of galls; but in the employment of this test certain precautions are necessary. For as an excess of potash may prevent the appearance of this precipitate, so also may a small excess of oxalic or tartaric acids prevent precipitation, or dissolve a precipitate already formed. A larger excess of citric acid seemed requisite for that purpose, and would also dissolve the gallat of columbium. In each case the precipitate may be made to appear by neutralizing the redundant acid; and for this purpose carbonate of ammonia should be employed: for although pure ammonia has no power of dissolving the oxide alone, yet the gallat seemed to be perfectly redissolved by that alkali.

When infusion of galls is poured upon the white oxide recently precipitated, and still moist, it combines readily, and forms the orange-coloured compound.

Prussiate of potash occasioned no change in an oxide that had been purified by a second fusion with potash; but it appeared to dissolve a small portion of the oxide, as infusion of galls, poured into the clear liquor, occasioned a cloudy precipitate of an orange colour, though no such precipitate took place when the infusion was mixed with the same prussiate alone.

Hydrosulphuret of potash being added to the oxide, and heated upon it, impaired the whiteness of its appearance, and seemed to detect the remains of some impurity which had not yet been removed by other means; but no appearance indicated the formation of a sulphuret of columbium.

From a careful repetition of these experiments upon each of the oxides, I see no reason to doubt of their perfect agreement in all their chemical properties; but there is nevertheless a very remarkable difference in the specific gravities of the two minerals from which they are extracted.

The specific gravity of columbite was ascertained by Mr. Hatchett to be 5·918; that of tantalite was found by M. Ekeberg

Ekeberg to be 7·953; and I have every reason to suppose their results correct, since a small fragment of the former appeared upon trial to be 5·87, while a specimen of tantalite, weighed at the same time, was as much as 7·8. I should, however, observe, that the specific gravities of three other fragments borrowed for this purpose were not so high, that of one being 7·65, of another 7·53, and of a third so low as 7·15.

It is evident that no variation of mere proportion of the ingredients can account for an increase of specific gravity from 5·918 to 7·953, which are in the ratio of 3 to 4; for since columbite contains four fifths oxide, if the whole remaining one fifth part in weight of that oxide could be supposed added to the same bulk, without diminution of the quantities of iron and manganese, the specific gravity would not then exceed 7·1; and even if a weight equal to one third of the whole were thus added, without increase of bulk, still the aggregate would not quite equal the heaviest tantalite in specific gravity; but, on the contrary, the quantity of white oxide in this specimen certainly does not amount to six sevenths, and probably is not more than five sixths of the whole mass.

The only chemical difference, by which this circumstance could be explained, would be the state of oxidation, which my experiments cannot appreciate; but it may also arise in part from actual cavities in the mass of columbite, and in part from the state or mode of aggregation.

XXXV. *Description of a Method of fitting up in a portable Form* The Electric Column *lately invented by* J. A. De Luc, *Esq. Also an Account of several Experiments made with it. By* B. M. Forster, *Esq.*

To Mr. Tilloch.

Sir, Having been informed that a row of galvanic plates had been constructed without any fluid being interposed, and that it acted very sensibly on a gold-leaf electrometer, I formed one, of about two hundred small circles of zinc, and the same number of blotting-paper and Dutch gold-leaf, the Dutch leaf being cemented on the paper with a solution of gum arabic; the blotting-paper was double, two pieces were gummed or pasted together before the Dutch leaf was put on. Through these circles, or plates, a silken string was passed for connecting them together. This small instrument

ment acted sufficiently powerfully on a very delicate gold-leaf electrometer, to encourage me to make a row consisting of a greater number of plates. To the two hundred I added about three hundred more, using, instead of the Dutch metal, silver-leaf, and inserted the whole in a glass tube fitted up with brass caps, screws, and balls. The instrument thus fitted up may be called an *Electric Rod.* I have some of these rods with the plates not connected by a string through them; which, provided the glass tube is very nearly of the same diameter as the plates, may be the best way of placing them; but unless the tube fits accurately the other mode will probably be found preferable, as the plates can be more easily placed regularly.

The Dutch metal, or silver-leaf, may either be fastened to the paper with gum, or paste made over the fire with flour and water.

The following experiments were made with a rod of five hundred series of plates,—whether with the one in which were two hundred plates of Duch metal, or in which there was none, but silver-leaf instead, it is not necessary to mention.

21st Sept. 1809. One leaf of an electrometer made of Dutch metal kept flapping to and from the side of the glass many times, when connected with the electric rod.

———— The ends of the rod being placed upon two electrometers,—when the top of either of them was touched, the electrometer at the opposite end diverged more immediately.

22d Sept. The rod was placed at the bottom of an electrometer; one leaf was attracted to the side and flapped several times.—This experiment shows that the electric power of these piles or columns acts through a portion of air: I held the upper part of the electrometer in my hand during this experiment.

24th Sept. A *small* piece of Dutch metal was attracted up to the ball at the zinc pole of the rod, and adhered to it.

4th Oct. A very light ivory needle, turning on a point (like a magnetic needle), was attracted by the rod; when a finger or a key was placed near one end of the needle, and the ball at the end of the rod also near the same end on the opposite side, the needle vibrated backwards and forwards. The needle was insulated, I believe, by a piece of amber.

———— The needle, after having been touched by the silver-end pole, evidently receded from that pole; or, as it is commonly called, was repelled, having been charged with

with the same kind of electricity as that end of the rod possessed :—the same effect was perceived when charged by the zinc pole.

——— One leaf of an electrometer (Dutch leaf) moved, when one of the balls on the rod was placed over the top, without being in contact with it.

15th Oct. The ivory needle vibrated between the balls of two rods, one of which was at the zinc pole, the other at the opposite pole.

One column which I have made, consists of about 500 plates, each about ¼th of an inch in diameter. I have put at the zinc end a piece of cork cut like the head of a snake or eel, and at the other end another to resemble a tail. This column may be called an artificial electric eel (*Gymnotus electricus*) : it is not inserted in a tube like the others, a silken string runs through the centre of the plates, which may be drawn tight ; then wound round a pin which is in the mouth, or may be loosened if desirable. This eel acts powerfully on the electrometers. The power appears to me to vary much more than that of the columns in tubes : provided the outside of these tubes be dry, I do not know that the strength of their electric power changes.

18th Oct. Three rods, each of 500 series, were supported upon insulated stands, and a plate of copper suspended at the silver pole of the combined apparatus ; another plate was placed under this, (as in the common electrical experiment of the dancing images,) one very small piece (or more) of tissue-paper was attracted up and fell down, and a little image of the same paper reared up, and once remained suspended to the upper plate, but I could not make it dance up and down.

22d Oct. One ball or both ? of Cavallo's pocket electrometer diverged, when three rods were combined ; the pith-balls are on wires. With these three rods I could not perceive the metallic taste in the mouth, which is so perceptible even with a single piece of zinc and silver placed against the tongue. When the ball or balls ? of the electrometer moved, the opposite end of the apparatus was touched. A small pith-ball, suspended I believe with a *single* thread of a silk-worm, vibrated between two fixed pith-balls, one of which was connected with the apparatus, the other communicated with the table.

23d Oct. A coated jar had a slight charge given to it with one of the electric rods. When the zinc pole charged the inside of the jar, that side gave signs of a *minus* state (as it is called), and the outside a *plus* state. This was shown by

by an electrometer, the leaves of which *diverged* when excited amber was holden near it, after it had received electricity from the inside, and *converged* when electrified from the outside of the bottle. From the usual effects of Galvanism the contrary might have been expected; that is, that the zinc end of the column would have produced the *plus* state, and the silver end the *minus*.

24th Oct. With three rods combined, a small brass ball suspended by silk between two bells, vibrated between them, causing them to ring: the bells were suspended from the ends of the apparatus. The next day, 25th Oct. (the Jubilee day), having fixed on glass pillars two bells, and hung by silk a brass ball from the upper part of a piece of wire, I placed the bells in connexion with the ends of the combined apparatus, by means of bent wires laid on them: the apparatus and bells were left for near an hour, during which time the bells kept ringing, at times stopping for a short interval, then ringing again; the clapper sometimes was seen to rest against one of them, then appeared to be disengaged by a person moving in the room. Whether the disengagement was always owing to some slight shaking of the table, or whether it was sometimes in consequence of the ball having acquired electricity, and then being repelled, I am not quite clear. It appears not improbable but that the weight of the clapper may be so adapted to the power of the apparatus, as to cause small bells to continue ringing for several years without intermission; if so, we shall have a machine which by those who do not consider the subject philosophically will be called *a perpetual motion*. How long the column will continue to produce the electric fluid cannot at present (or perhaps ever) be determined. The principal difficulty to be overcome, in order to keep the clapper in continual motion throughout the different seasons of the year, appears to be the want of a very accurate *insulation* of the apparatus; for, if the glass tubes or the pillars which support them are damp, the current of the electric fluid will not pass along in the proper direction for the experiment.

29th Nov. Five rods, each of 500 series, were combined; with these, two small bells kept ringing on and off for more than four hours, part of which time I was not in the room, so cannot tell how often they might have stopped: the ringing sometimes began again evidently not from any shake, but I imagine from the clapper having become electrified, and then being (as it is usually called) repelled from the bell against which it rested. I placed three rods of 500 series

series (insulated,) in a box, and brought wires from the ends of this combined apparatus, which I made communicate with two bells. I placed, on Tuesday 27th February 1810, this apparatus in a closet, where I left it until Sunday 11th March, the bells continuing to ring (as far as my observations went) from the time they were put into the closet until that day; when they ceased. What was the cause of their stopping I do not know, but imagine it was owing to dampness. I cannot ascertain that they rang the whole time without stopping, but have no reason to believe otherwise. I intended some months ago to have sent you the description of the above-mentioned apparatus with experiments, but deferred sending it on account of Mr. De Luc's paper not being published, which he sent to the Royal Society in March, last year, and which contains a description of the *electric column* and its properties. He hopes soon to publish it himself. In the mean time he has permitted me to communicate my account to you. I consider the invention of this column as the most important discovery in the science of electricity since that of the *Voltaic pile*, and do not doubt but that when Mr. De Luc gives his paper to the public, it will prove extremely interesting, and I have reason to believe it may lead to further discoveries which will be considered as very important in this branch of science.

Description of the Figures.

Fig. 1. A column of 500 series of plates about $\frac{5}{8}$ths of an inch diameter, in a glass tube with brass caps, &c.

AA Brass caps, the ends of which screw on and off.

BB Screws which pass through the caps into the tube, having a brass plate at the end for compressing the electric column.

CC Balls at the outer ends of the screws.

Fig. 2. One of the screws with a ball at the outer end, and a brass plate or circle at the other. A small hole is pierced in this plate, through which the silken string connecting the electric column may pass.

Fig. 3. A smaller column (the plates of which are about $\frac{1}{4}$th of an inch diameter,) fitted up with a cork head and tail to represent an eel.

Fig. 4. Size of plates in larger column.

Fig. 5. Ditto of the smaller one.

Fig. 6. A combined apparatus, consisting of three rods resting on insulating stands, and having a wire projecting from each of the outer rods: these

wires are for connecting the apparatus with electrometers, &c.

Fig. 7. A couple of small bells supported on glass pillars, with a brass ball suspended from a wire, used as a clapper to ring the bells.

On Wednesday night, 14th March, I put into a closet a couple of bells, communicating with the three rods above mentioned in a box; they then began to ring, and are now ringing:—how long they will continue so I cannot say, perhaps some change in the weather may soon occasion the clapper to cease vibrating.

I remain, &c.

B. M. Forster.

Walthamstow, Essex, 20th March 1810.

XXXVI. *On the comparative Influence of Male and Female Parents on their Offspring.* *By* Thomas Andrew Knight, *Esq., F.R.S.* *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.**

My Dear Sir, I have been engaged, during many years, in experiments on fruit-trees, of which the object has been to discover the best means of forming new varieties, that may be found better calculated for the climate of Britain than those at present cultivated. In this inquiry my efforts have been always most successful, when I propagated from the males of one variety and the females of another; and I was enabled, by the same means, to ascertain more accurately, than had previously been done, the comparative influence of the male and female parent on the character of the offspring. The analogy that subsists between plants and animals, in almost every thing which respects generation, induced me also to attend very minutely to similar experiments in which I engaged on some species of animals; and as the repetition of such experiments would necessarily require a very considerable space of time, and as the results seem to lead to conclusions that may be of public utility, I have thought the following account sufficiently interesting to induce me to address it to you.

Linnæus conceived, that the character of the male parent predominated in the exterior parts both of plants and animals; and the same opinions have been generally entertained by more modern naturalists. But the Swedish

* Philosophical Transactions for 1810, Part II.

philosopher

philosopher appears to have been misled, by the striking predominance of the character of the male parent in male animals, and to have drawn his conclusions somewhat too generally: for I have observed that seedling plants, when propagated from male and female parents of distinct characters and permanent habits, generally, though with some few exceptions, inherit much more of the character of the female than of the male parent; and the same remark is applicable, in some respects, to the animal world, as I shall point out in the succeeding narrative.

My experiments were made on many different species of fruit-trees; but most extensively, and under the most advantageous circumstances, on the apple-tree: and as the results were all in unison with each other, it will be necessary to trouble you only with an account of some of the experiments which were made on that species of fruit-tree.

The apple, or crab of England, and of Siberia, however dissimilar in habit and character, appear to constitute a single species only; in which much variation has been effected by the influence of climate on successive generations: for the two varieties readily bred together, and the offspring, whether raised from the seeds of the Siberian or British variety, were prolific to a most exuberant extent. But there was a very considerable degree of dissimilarity in the appearance of the offspring; and the leaves, and general habits of each, presented an obvious prevalence of the character of the female parent. The buds of those plants, which had sprung from the seeds of the cultivated apple, did not unfold quite so early in the spring; and their fruits generally exceeded, very considerably, in size, those which were produced by the trees which derived their existence from the seeds of the Siberian crab. There was also a prevalence of the character of the female parent in the form of the fruit; but the same degree of prevalence did not extend to the quality and flavour of the fruit; for the richest apple that I have ever seen, and which afforded expressed juice of much higher specific gravity than any other, sprang from a seed of yellow Siberian crab.

The prevalence of the character of the female parent in the preceding cases, may possibly be suspected to have arisen from some error, or neglect of accuracy in making the experiments; but I do not conceive that any such errors could have existed; for the trees of each variety were trained to walls, where they blossomed much before any

 others

others of the same species, and the stamina were always carefully extracted, whilst immature, from every blossom which I intended to afford seeds. The remaining blossoms of the trees were also totally destroyed, and no other blossoms, except those from which the pollen was taken, were ever unfolded in the neighbourhood, in the season when the experiments were made; and I have also invariably declined to draw any conclusion from the appearance of a plant, in which I could not certainly distinguish some portion of the features and character of the supposed male parent.

It is perhaps also proper to state, that the predominance of the character of the female parent, could scarcely have arisen from any defective action of the pollen; for, except in cases where superfœtation took place, I have invariably found the effect of a very large, or a very small quantity of pollen, to be invariably the same, in its influence on the offspring; and in the greater part of the experiments, from which I have drawn the preceding conclusions, more than ten times as much pollen was deposited on the stigmata, as could have been deposited in unmutilated blossoms by the ordinary means employed by nature.

In all attempts to discriminate the different influence of the male and female parent on the offspring of animals many difficulties present themselves, owing to the intermixtures which have been made of the different breeds of domesticated animals of every species, and the consequent absence of all hereditary permanency in the character of each variety. For, under these circumstances, the offspring will be very frequently found to show little resemblance either to its male or female parent, either in form, or stature, or colour. It will therefore be necessary, before I enter on the subject of viviparous animals, to observe that when I apply the terms large and small to the male or female parent, I extend the meaning of those terms to the parentage from which the male and female descend, and not to the size of the individual only, which becomes the immediate parent of the offspring.

Mr. Cline has observed, in a communication to the Board of Agriculture, that if the male and female parent differ considerably in size, the dimensions of the fœtus, at the birth, will be regulated much more by the size of the female than of the male parent; and, if the meaning of the terms large and small be extended to the varieties, as well as to the individuals, his remark is perfectly just. But

experience

experiençe compels me wholly to reject the inference that he has drawn respecting the advantages of propagating from large, in preference to small females.

Nature has given to the offspring of many animals (those of the sheep, the cow, and the mare, afford familiar examples) the power, at an early age, to accompany their parents in flight; and the legs of such animals are very nearly of the same length, at the birth, as when they have attained their perfect growth. When the female parent is large, and the fœtus consequently so, the offspring will be large at its birth, in proportion to the bulk it will ultimately attain, and its legs will thence be long comparatively with the depth of the chest and shoulders. When, on the contrary, the female is small, and the fœtus so, at the birth, the length of the legs of the young animal will be short comparatively with the depth of its chest and shoulders; and an animal in the latter form will be greatly preferable, either for the purposes of labour, or of food to mankind. I have seen this difference in the influence of the male and female parent, on the offspring, very strikingly exemplified, in the result of an attempt to obtain very large mules from the male ass and the mare. The largest females, that could be procured, were selected, and the forms of the offspring, at the birth, were perfectly consistent with the theory of Mr. Cline; they were remarkably large: and I observed, that the length of their legs, when they were only a few days old, very nearly equalled that of the legs of their female parents. I examined the same animals when five years old, and in the depth of their chests and shoulders they very little exceeded their male parent; and they were consequently of little or no value; whilst other mules, which were obtained from the same male parent (a Spanish ass), but from mares of small stature, were perfectly well proportioned. I have never seen the little mule, which is propagated from the female ass and the horse, nor even a delineation or description of its form; but I do not entertain any doubt that its chest and shoulders are excessively deep and strong, comparatively with the length of its legs, and that, on account of this peculiarity in its form, it has been so frequently shown on the Continent, under the name of a jumart, as the pretended offspring of the mare and the bull.

In opposing the theory advanced by Mr. Cline, it is not by any means my intention to enter the lists with him, as a physiologist; but, as a farmer and breeder of animals of different species, I have probably had many advantages,

 which

which he has not possessed; and my conclusions have been drawn from very extensive, and, I believe, accurate observation.

There is another respect in which the powers of the female appear to be prevalent in their influence on the offspring, and that is relative to its sex. In several species of domesticated, or cultivated animal (I believe in all), particular females are found to produce a very large majority, and sometimes all their offspring, of the same sex; and I have proved repeatedly, that, by dividing a herd of thirty cows into three equal parts, I could calculate, with confidence, upon a large majority of females from one part, of males from another, and upon nearly an equal number of males and females from the remainder. I frequently endeavoured to change these habits by changing the male; but always without success; and I have in some instances observed the offspring of one sex, though obtained from different males, to exceed those of the other, in the proportion of five or six, and even seven to one. When, on the contrary, I have attended to the numerous offspring of a single bull, or ram, or horse, I have never seen any considerable difference in the number of offspring of either sex. I am therefore disposed to believe that the sex of the offspring is given by the female parent; and the probability of this seems obvious in fishes, and several other species of animals which breed in water; and though the evidence afforded by the facts adduced is not by any means of sufficient weight to decide the question, it probably much exceeds all that can be placed in the opposite scale.

In oviparous animals, I have had reason to think the influence of the female parent quite as great as amongst the viviparous tribes, though my observations have been more limited, and less conclusive. In viviparous animals, the size of the fœtus is affected by the influence of the male parent, and, in some instances, not inconsiderably; but the size and form of the eggs of birds do not appear to be in any degree changed or modified by the influence of the male; and therefore the size of the offspring, at the birth, must be regulated wholly by the female parent; and this circumstance permanently affects the form and character of the offspring. The eggs of birds, and those of fishes and insects (if such can properly be called eggs), appear to resemble the seeds of plants, in having their forms and bulk wholly regulated by the female parent; but nevertheless their formation appears to depend on very different laws. For the eggs, both of birds and of fishes and insects, attain

their perfect size in total independence of the male, and the cicatricula, the vitellus, and the chalazæ have appeared (I believe) to the most accurate observers, to be as well organized in the unimpregnated as in the impregnated egg: in the seed, on the contrary, every thing relative to its internal organization appears dependent on the male parent. Spallanzani has, however, stated, that many plants produced well organized seeds, and even seeds which vegetated perfectly, under circumstances in which it is not easy to conceive how the pollen of the male plant or flower could have been present. But the Italian naturalist appears to have blundered most egregiously in his experiment; or (which I conceive to be more probable) he became the dupe of the refined malice of his countrymen; for, I repeated his experiments under very favourable circumstances, and with the closest attention, but I failed to obtain a single seed. The gourd alone produced apparently perfect fruit, and the seed-coats acquired their natural size and form; and in this respect the growth of its seeds appeared to be, like that of eggs, wholly independent of the influence of the male. But the *seed-coats* of the gourd were perfectly *empty*, and I could not discover, at any period of their growth, the slightest vestige either of cotyledons, or plumule, nor of any thing that appeared to correspond with internal organization of a seed of the same plant, under different circumstances. Spallanzani has not, I believe, mentioned the species of gourd upon which he made his experiments: the common, or orange gourd of our gardens, was the subject of mine.

In comparing the mode of the formation and growth of eggs with the observations I had previously made on the growth of seeds, I have been favoured with the very able assistance of Mr. Carlisle, for which I have on this, as on many other occasions, to acknowledge much obligation.

I am, my dear sir,
with great respect, sincerely yours,
Downton, May 20, 1809. THOMAS AND. KNIGHT.

XXXVII. *On the Fossil Bones of Horses and Wild Boars.* *By* G. A. CUVIER*.

THE above are the only species of animals which remain to be described, in order to complete the history of qua-

* *Ann. du Muséum*, tome xiv. p. 33.

drupeds with hoofs, which have been found in the fossil state; and our task will be the easier, as they have been dug up from loose soils only, for the most part of recent formation; and such of their remains as have been collected, cannot enable us to distinguish them from living species.

Article I. *Fossil Bones of Horses.*

These are as common in loose strata as the bones of any other large animal, and yet little mention has been made of them in works on fossil bones; either because their presence was regarded as a very simple occurrence, not deserving of attention, or because they were not recognized as being the bones of horses.

There are various proofs of this last oversight, which would appear very extraordinary if we were not aware how superficially fossils and petrifactions have been examined.

Thus we find in the *Traité des Monstres* of Aldrovandus, published by Bernier, p. 37, two horse's teeth given as the teeth of giants, while in the *Museum metallicum* of this author, published by Ambrosinus, p. 830, teeth of the same animal are represented correctly.

In another memoir we have said that Lang, in his *Historia Lapidum figuratorum Helvetiæ*, tab. XI. *f.* 1, 2, had taken a horse's tooth for the tooth of a hippopotamus.

We may add that Kundmann has engraved others, without knowing what to make of them (*Rar. Nat. et Art.* tab. II. f. 4 and 5); and that Walch, who had received them from Quedlimbourg, confines himself to remarking their resemblance with those of Lang and Kundmann, without endeavouring to determine them more precisely (*Monumens de Knorr*, II. sect. II. page 152).

The number of authors who have been more adventurous is very small; such as Bourguet, who quotes a single jaw-tooth found at a depth of 60 feet, on digging a well near Modena (*Traité des Pétrifications*); and Romé de l'Isle, who reckons in the number of the subjects in the Cabinet of Davila, a fossil horse's tooth in its alveolus, near Canstadt. (*Cat. de Davila*, III, page 230).

It is certainly to this silence of most naturalists, with respect to the fossil bones of horses, that we are indebted for the silence preserved by M. Faujas on the same subject in his *Géologie*, although he might have taken great advantage of it to support his favourite opinion respecting the identity of fossil animals with those of the present time.

In fact, the fossil bones of horses cannot be distinguished from the bones of living horses; and nevertheless we find them

them most assuredly in the same strata which contain unknown animals.

We have already said that there were thousands of horses' teeth in the celebrated depôt of bones of elephants, rhinoceroses, tigers, and hyænas, discovered in 1700 near Canstadt in Wirtemberg: their association with the elephants seemed to be a general occurrence.

We have seen with our own eyes hundreds of bones and teeth of horses dug up from the canal of the Ourcq, in the spot from which elephants' bones also had been taken; and among the horses' bones there were some completely petrified.

In the quarry of Fouvent le Prieuré, in the department of the Haute-Saone, from which bones of elephants and of hyænas have been procured, several bones and teeth of horses were at the same time found, which have been also sent to our Museum.

M. de Drée is in possession of a piece of a horse's jaw found at Argenteuil, nearly in the same spot with an elephant's jaw.

M. Fabbroni has sent me drawings of several similar portions dug up in the upper Val d'Arno, with bones of elephants, rhinoceroses, and mastodonti with straight teeth.

Finally, M. Fischer has procured me some drawings of horses' teeth brought from the Bergstrasse, and placed in the Cabinet of Darmstadt.

I am convinced from these observations, that if we have not more frequently heard of horses' bones dug up with those of elephants, it has arisen from the former having been regarded as less interesting.

We shall not repeat what we have said of those which we sometimes find in osseous strata: but it is in recent alluviations that most of them are found, as might reasonably be expected.

There is scarcely any valley into which we can dig in any direction, without finding horses' bones in the depositations made by rivers: the valley of the Seine, that of the Somme, and without doubt several others, are full of them.

M. Traullé sent me several specimens from the banks of the Somme; and I have seen them myself dug up from the foundations of the bridge now constructing opposite the Military School.

These last are not interesting, because they have been deposited since our continents assumed their present form: the former, however, being those which accompany the bones of elephants and tigers, are of an anterior order of things.

things. Did the horses to which they belong resemble those of the present day in every respect?

I must confess that comparative anatomy cannot answer this question.

I have carefully compared the skeletons of several varieties of the genus *Equus*, those of the mule, the ass, the zebra, and the couagga, without being able to find in them a character sufficiently fixed to entitle me to hazard a decision. If we could obtain an entire fossil head, we might perhaps set on foot some comparison; but with the other bones, most of which are mutilated, we can obtain no result.

We may therefore rest assured, that one species of the horse genus served as a constant companion to the *elephants* or *mammoths*, and to the other animals of the same æra, the bones of which fill our large basins; but it is impossible to say in what respects they resembled any of the species known at present.

It only now remains to point out the characters by which we may distinguish the bones of horses. As it is with the ox and the buffalo that they are most liable to be confounded, it is with these that we must compare them.

The upper grinders of horses are prismatic, like those of the ox and buffalo, and marked in the same way with four crescents; but they have besides a fifth, in the midst of the inner edge. The lower grinders are more compressed, and have four crescents in the horse as well as in the ox; but instead of being parallel in pairs, they are alternate, the first of the inner edge corresponding to the interval of the two of the outer edge.

The shoulder-blade of the horse has its spine more elevated at the upper third part of it, and decreases from thence to the acromion. In the ruminating animals there is also an elevation at the same spot; but it is at the lower extremity, and at the acromion, that the spine is most prominent.

In the humerus of the ox, the great tuberosity rises far above the rest of the upper head, and there is only a groove for the biceps humeri; in the horse this tuberosity does not rise more than the rest, and there are two different grooves in front.

The camel and other ruminants resemble the horse more than the ox in this respect.

The cubitus of the ox, although attached to the radius, may be distinguished throughout its whole length; that of the horse is entirely lost from its superior third part, being only marked afterwards by a kind of thread.

The

The lower head of the radius of the horse is divided into two facets, by an almost perpendicular ridge; that of the ox is divided into three, by two very oblique ridges.

The ox has one bone less in the carpus than the horse, because its os trapezoides is confounded with the great bone. Every person is acquainted with the difference of their metacarpus and their hoofs.

The ischion of the ox has its tuberosity higher than that of the horse, and the os ileum of the latter, on the contrary, is higher at its upper angle: this occasions the striking difference in the crupper of the two animals.

The femur of the horse has three trochanters; that of the ox has only two, and the great trochanter is less elevated.

The lower head of the tibia of the ox is rectangular, and has at its inner edge a facet for the articulation of the fibula; that of the horse is very oblique, and almost triangular.

The same difference of obliquity is discernible in the astragali: that of the horse, besides, has but a very small facet for the os cuboides; that of the ox rests on this bone nearly the half of its inferior head.

The os scaphoides of the horse is much larger than its cuboides, and remains always distinct from it: in the ox these two bones are equally large, and are always compounded. The horse has only one os cuneiforme, and the ox has two.

The differences of the metatarsus and of the hoofs, which have occasioned those of the tarsus, are known to all naturalists.

By means of these short and simple characters we may easily distinguish the bones of the extremities of the two species.

Each of the vertebræ, separately examined, would also furnish characteristics; but the detail would be endless, and it is very rare that we find vertebræ isolated from other bones. I think I have now furnished geologists with all that is requisite.

Article II. *Of the Fossil Bones of Wild Boars.*

I do not find many indications of the teeth of these animals in authors: all those that I have seen, came from peat mosses, or other recent soils; and I do not know if they ever accompanied the bones of elephants.

Walch mentions the vertebræ of a petrified hog, alluded to by Luid, and after him by Argenville; but we cannot trust to such authors or their descriptions. Gmelin, Wal-

lerius,

lerius, and others whom I have consulted, do not speak at all of this kind of fossil.

There is nevertheless in the *Museum Beslerianum*, plate XXXI, a piece of the fossil tooth of a wild boar, under the singular name of *pseudo-corona-anguina*; and Grew says that the cabinet of the Royal Society of London has similar specimens : but neither of these authors assigns the origin or the species.

M. Delaunay, in his *Mémoire sur l'Origine des Fossiles accidentels des Provinces Belgiques*, p. 36, relates, that in the environs of Alost, on digging into a moss, " they found the osseous part of a wild boar unknown in Europe, and they considered what must have been the extraordinary size of the animal when alive." He adds, that what made the animal be recognized, " were the tusks, of a length in every respect astonishing." It would have been very easy to have added the length of these tusks, and some figure or description of this head. But geologists have rarely descended to what they considered as minutiæ, and preferred spending their time in contriving systems, to employing it in accurate researches : thus the above fact, which might have been made interesting, is totally useless.

For my part, I have some teeth of wild boars which seem to have remained long in the earth. I have some also stained black by the moss in which they certainly had been immersed ; but I am not acquainted with the precise origin of any of them, except of a tusk found on digging the foundations of the bridge of Jena, opposite the Military School, with several bones of horses, pieces of boats, and other artificial fragments. I have also a piece of a jaw brought from the mosses in the department of the Oise, deposited in the cabinet of the School of Mines. Both are therefore of very recent strata, and they do not differ in the least from the living analogy.

Adrian Camper sent me the drawing of the lower half of the humerus of a hog or a wild boar, which had been transmitted to him from Hartz, but as to its precise position he knows nothing certain.

The head of the *Sus* genus is so easily distinguished from all others, that we have no occasion to give its characters.

Its grinders represent on a small scale those of the mastodontus with straight teeth, having also blunt tubercles furnished on their edges with smaller tubercles.

In the wild boar, domestic pigs, Siam pigs, and Madagascar wild boars, the natural and complete number of grinders is seven.

The

The posterior lower grinder has five groups of tubercles; that of the upper has six. The eight which precede them have each four groups ranged in pairs. The fourth on each side has three groups ranged in a triangular form; and the three anterior, having their tubercles on a single line, are almost sharp-edged.

The anterior tooth falls very early in our European pigs; and I never found it in the babiroussa, the number of which should be six, but it is frequently five only, in consequence of the casting of the anterior tooth: I found but six also in two peccaris.

The wild boar of Ethiopia has only three teeth, all composed of cylinders tied together like the laminæ of those of the elephant, and presenting circles at their surface when they are filed down. They are very unequal; for the last has no less than twenty-three circles ranged in three lines.

Every species has its peculiar form of tusks; but all the tusks and all the grinders which I have observed, were similar to those of the common wild boar.

The extremities of the *Sus* genus have a great resemblance to those of the ruminating animals. As it is most likely that the bones of the former may be confounded with those of stags and sheep, it is with these last that we ought to compare them.

The shoulder blade, like that of the horse, has the spine flattened in front, and more prominent in the upper third part, where it forms a hook bent backward.

The great tuberosity of its humerus is very high, as in the sheep; but it becomes broader behind, and is followed by a broad reentering arc.

The cubitus is very broad and distinct throughout its whole length; the greatest part of it in the sheep is attached. In the stag it is at least much more slender.

The carpi have a close resemblance, with this difference, that the trapezoidal bone is distinct in the *Sus* genus, whereas it is close in the ruminating animals; and the unciform bone is narrower, whereas the os scaphoides is broader. The differences in the femurs are almost incapable of description in words; but the tibia may be recognized because it is shorter; its inferior head is square, and does not decrease from back to front, and has no articulation for the fibula. The chief difference of the tarsus depends on the small cuneiform bone, on the sole of the fifth toe, and on the os scaphoides remaining distinct from the cuboides. As to the metacarpi, the metatarsi, and the toes, they cannot be confounded.

XXXVIII. *Ex-*

XXXVIII. *Extract from a Memoir by M.* Mathieu, *on the Discovery of several Blocks of orbicular Granite recently found in Corsica* *.

The insulated block of orbicular granite which was found in Corsica in 1785, on the small plain of Talavo, half a league from the sea, on the shores of the Gulf of Valinco, on the Istria road, not far from a place called la Stanzona, and which Messrs. Sionville and Barral were the first to describe, fixed the attention of mineralogists, from the singular form assumed in this rock by the white semi-transparent feldspar and the *amphibole* or *horne blende* of a deep black, a little greenish, arranged in several concentric circles, which had given rise to species of round or oval bowls, immersed in a confused mixture of the same two mineral substances which form the basis of the rock.

This stone, so singular by the system of its formation, and by the effect which it produced when polished, made it very much sought-after for cabinets, and it soon became rare and of high price.

In vain did Messrs. Barral, Sionville, and after them Dolomieu, Besson, and several other mineralogists, make inquiries after the rock which had given birth to the insulated and partly smooth block, which some convulsion had transported and buried in the small plain of Talavo;—all their pains were useless.

Notwithstanding their want of success, a Corsican naturalist, M. Rampasse, was more fortunate; because his knowledge of the language and manners of the inhabitants of the mountains admitted of his pursuing, with a hammer in his hand, the chain of mountains from which he presumed that the block of the orbicular rock of Talavo must have been torn at a very remote period by the action of the sea.

M. Rampasse has executed this arduous journey, during which he collected a fine series of rocks and other minerals; but he was not more fortunate than the rest with respect to the globular granite. His inquiry, however, made us acquainted with the position of a porphyritic rock crowded with globular bodies, in general larger than those of the stone found at Talavo; and the formation of which, without being absolutely the same with the latter, nevertheless resembled it considerably; but the rock, of a differ-

* *Annales du Muséum d'Histoire Naturelle*, tome xiv. p. 82.

ent colour, was softer, and did not receive the brilliant polish of the orbicular granite. M. Rampasse brought to Paris some magnificent specimens of this porphyry with large globules. This variety was not to be found in any cabinets.

It results from these details, that all hopes were given up of other masses of granite similar to that of the plain of Talavo, when a particular circumstance brought to view several other blocks, which are said to exist in their native position, a leage from Talavo, and resting on the same rock which gave birth to them.

It is to M. Mathieu, captain in the imperial artillery, and commanding at Ajaccio, that we owe the first information on this discovery, consigned in a manuscript memoir, accompanied with a topographical plan, and a very excellent specimen of this granite, which is of precisely the same form with that which was formerly discovered.

"This superb production," says M. Mathieu, "is found in considerable masses on the estate of Sartene, the property of M. Jean Paul Roccaserra: its present situation is about three fourth parts up a very steep mountain, from which it has been insulated by accident; it is in *blocks smoothed* (arrondis) *in consequence of decomposition*, which blocks are comprised within a space which does not extend beyond four hundred square metres. The base is a granite composed of semitransparent quartz, of amphibole with large crystals, and of mica in a small quantity: sometimes shades are discovered which give a feeble appearance of the globulous system. The rest of the mountain is, like those adjacent, composed of a granite of quartz, feldspar, and mica."

M. Mathieu adds, that the lichens and mosses which covered the blocks of this new orbicular granite and concealed its characters, did not permit those who visited the same mountain to see that the discovery was owing *to the recent separation of two parts of one block.* The distance of the position of the ancient block of Talavo from the Rizenare, a river which washes the foot of the mountain on which the recent discovery has been made, is one myriametre and a half. M. Mathieu does not think that this river has ever been capable of transporting this old block of granite to such a distance; and he is perfectly right: but when he presumes that, "*in very remote ages, this same block has been discovered in the Rizenare, and thence transferred by the care of an architect to the spot where it was to be cut with the chisel,*"—this conjecture does not seem to rest

rest on any solid foundation: and it would be entirely refuted if the *smooth blocks*, alluded to by M. Mathieu, discovered within the space of 400 square metres, instead of having been *worn or smoothed by the effects of decomposition*, had been so worn by the action of some violent convulsion of the sea at very remote periods, when these blocks as well as that at Talavo must have been transported to the places where they were discovered. We see many examples, even on very high mountains, of these accidental transports of masses much more numerous and much larger, of granites and other rocks not less hard, all the angles of which have been flattened by frictions. Besides, those in the environs of Sartene are at a trifling height, and of small size, in comparison with the enormous masses mentioned by Saussure in his Travels in the Alps, and which he considers as the result of what he calls *la grande débacle.*

We are certainly far from wishing to lessen in the smallest degree the merit of M. Mathieu's Memoir, to whom we are under great obligations for communicating the discovery; but we insist the more strongly on a new examination of the smoothed blocks of Sartene, as the precise knowledge which we have of that of Talavo, which has never been *smoothed* by *decomposition*, but by *friction*, leads us by analogy to consider the blocks in question as being the result of a similar cause:—for nothing could have less tendency to decomposition than the orbicular granite of Talavo, the block of which was very hard and very sound internally, as well as on the external surfaces, which last did not bear any other signs of destruction than that produced by shocks or by friction.

We must therefore invite M. Mathieu to examine again with attention the state of the blocks which he mentions in his Memoir, and to ascertain positively, First, If they really were *smoothed by the effects of decomposition?* Secondly, If these blocks are adherent, or separated from the granite rock on which they rest? which is not clearly enough described in his Memoir. And finally, If semitransparent quartz exists, as he says it does, in the new orbicular granite? for the old piece is devoid of it, and is composed only of semitransparent feldspar, and amphibole of a greenish black, with a little mica, which is only met with rarely and in small spots.

XXXIX. *Pro-*

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

The meetings on the 1st, 8th, and 15th of March were occupied in reading Dr. Herschel's supplementary remarks and illustrations to his former papers on the nature of coloured concentric rings seen between object-glasses. The details were so unconnected in themselves, and referring so intimately to his preceding observations and the objections which have been made to them, that it is not possible to convey any just idea of the author's experiments in an abstract.

March 22, The right honourable President in the chair. A letter to the president from Mr. Knight was read, on the functions of the leaves of trees. In this short letter Mr. Knight merely confirmed the opinions and observations which he made on this subject several years ago, namely: that the leaves of trees perform the same functions as the lungs of animals; that grafts of trees perish as soon as the parent stock, from the inability of the leaves to perform their office, and support the increased circulation of a young and healthy stock; and that consequently a rich soil, and augmented nourishment to the roots of such trees, only tend to accelerate their decay instead of increasing their vigour, as more sap by such means is propelled to the branches than can be digested by the leaves. This theory of the perishable nature of grafts, which has met with much opposition, Mr. Knight now considers as sufficiently established on the sure basis of physical demonstration.

N. B. The doses of magnesia administered by Mr. Brande to his calculous patients * varied from 15 to 20 grains night and morning.

SOCIETY OF ANTIQUARIES.

An account of the earl of Cumberland's expedition to the Azores in 1589 was read to this society; but it contained no new facts of particular importance, except so far as it proved the early superiority of the British navy.

Part of a very accurate description of Rippon minster was also read, in which it was alleged that at least a wing of this church was built prior to the Norman conquest.

ROYAL INSTITUTION.

On Saturday, the 3d of March, Mr. Professor Davy began his second course of electro-chemical lectures, with an introductory

* See page 155 of our last Number.

troductory discourse, in which he explained the principle upon which it is proposed to new-model this institution. The plan appears to be so judiciously adapted to the circumstances of the establishment, and holds out so fair a promise of a successful prosecution of scientific researches, that we entertain no doubt of its meeting with the general approbation of the proprietary, and receiving that share of public encouragement to which it is so eminently entitled. After a concise historical survey of the origin and progress of the Institution, and of the important discoveries which had repaid the liberality of its founders, Mr. Davy pronounced a short and eloquent dissertation on the utility of philosophical inquiries in general, and proceeded to explain the nature of those improvements, which, he conceived, were calculated to render the Institution a more efficient instrument of public advantage.

The funds of the Institution, he said, had been inadequate to the expenses of the establishment; and some injury had been sustained from the purchase and sale of the proprietors' shares. It was not to be expected, that those who could make an interest in a scientific institution, an object of pecuniary profit, could ever feel a zealous solicitude for the advancement of science. It was therefore proposed that the shares, which originally conveyed an interest in perpetuity, should be converted into shares which conferred merely an unalienable interest for life. But lest any of the present proprietors should conceive themselves injured by the change, a fund was to be provided, by a loan, for their indemnification. Thus none would continue to be proprietors, but such as were animated by sentiments favourable to that description of patronage, from the beneficial influence of which the objects of the Institution would be most effectually cherished and promoted.

It might perhaps be a question, whether proprietors should then be admitted indefinitely, or restricted to a limited number, and appointed by election. Experience had shown, that what every one could attain, was scarcely thought desirable by any. But where any difficulty presented itself, it produced a desire to surmount it. Hence, if it became an object of some exertion to obtain the distinction of a proprietor, a class of candidates would probably present themselves for election, consisting of individuals to whom the pursuits of science are of real and intrinsic importance; and from such patronage the best results might be expected.

In order that the proprietors and the public might derive every

every benefit from the Institution, it was proposed, in addition to the advantages presented by the lectures, the use of the library, and the collection of mineralogy, to put them in possession of the result of all scientific investigation that might be pursued, as well as of the proceedings of the Institution, by a quarterly publication of its labours.

To one class of proprietors, namely, the possessors of landed property, the Institution might prove eminently useful. The value of the mineral productions of their estates might be ascertained, without exposing them to the impositions sometimes practised by adventurers, who, for the selfish purpose of promoting their own interests, recommend the working of mines, without any prospect of advantage to any one but the individuals employed upon the undertaking. Several instances, Mr. Davy observed, might be mentioned of benefit which had already accrued to persons, who, before engaging in expensive enterprises of this kind, had transmitted specimens of the productions of their estates to the Institution, in order that they might be analysed; and the report which was returned to them enabled them to avoid the unprofitable hazards to which such speculations are exposed. One gentleman conceived he had discovered a valuable coal mine on his estate. Upon examining the substance, however, it was at once ascertained that it was destitute of all bituminous properties, and the working of a mine of this quality would have been attended only with expense. Another gentleman supposed he had discovered on his estate a stratum of alumina; but, upon its analysis, it was found to be a clay of inferior value. Instances of this description might, if necessary, be easily multiplied. To landed proprietors, therefore, the Institution might prove eminently beneficial.

It had also its claim upon princes and statesmen for their support. Even the materials of war, which in these times may be of the highest importance to the state, might, from new results of scientific investigation, be essentially and effectively improved. But there is another point of view in which it has still a superior claim to their attention and patronage. With the progressive advancement of sciences and arts, the increasing prosperity of the country is closely and inseparably interwoven. Of this no one was more sensible than the great Colbert. He knew how to appreciate their importance, and cherished them with the utmost assiduity and care. Nor was he disappointed in the expectation of their natural effects; for the prosperity of France, in the reign of Louis the Fourteenth, was most rapidly ac-

celerated by the encouragement which this able minister and statesman judiciously extended to every branch of science and of art. In this country, too, we are indebted for a large proportion of our prosperity to the success with which science has been prosecuted. But how much might it have been augmented, if the arts and sciences had received an adequate degree of encouragement! Were it possible to appropriate to this object the funds collected in one year for charitable purposes, a foundation might be laid for advancing the prosperity of the country to an almost indefinite extent, and to a point, at least, which would enable us to bid defiance to the restrictive edicts, by which the enemy has vanly attempted to check the wealth and power of the British empire.

Nor are the minute details of science and of art unfavourable to the cultivation of eloquence. One of the greatest statesmen, and at the same time one of the greatest orators this country ever produced, owed, in a great measure, the variety, the charms, and the force of his eloquence, to the intimate knowledge which he had acquired of every branch of science and of art. These sources of intelligence supplied him with that copiousness of illustration, with which his orations were enriched, and enabled Mr. Burke to collect within the boundaries of his own genius, every thing that could adorn and embellish his elocution.

It has been supposed, said Mr. Davy, that this Institution may ultimately encroach on the province of some of the ancient and venerable establishments of the country, where ancient erudition has long been cultivated with success. This opinion, however, is founded in error. The precious remains of antiquity, which enlightened the darkness of the middle ages, and delivered down to us some of the most inestimable treasures of human knowledge, we can never value too highly. Let them continue to be the guides of our taste, and the beacons by which our course is to be directed. But let them not be exclusively studied. The pursuits on which such minds as those of Boyle, of Bacon, and of Newton, were employed, are of a much higher order of utility, and far more conducive to the interests and the happiness of mankind.

I presume too to hope, continued Mr. Davy, that the encouragement which the Royal Institution has hitherto received from its female visitors, will not be withheld from it under its new modification. It may afford them opportunities of acquiring that knowledge, which will contribute to render their elegant acquirements still more interesting.

By

By increasing the sphere of their intelligence, they will secure not only an addition to their own happiness, but the higher gratification of imparting to their children that useful instruction, which cannot fail to strengthen and to endear the relations of domestic intercourse.

Mr. Davy concluded a very eloquent and comprehensive introductory lecture, by observing, that discoveries in physical science are not to be estimated solely by their conduciveness to general utility. They produce on the human mind the happiest and the most sublime impression, in proportion as they develop the harmony and simplicity which reign throughout the works of that Being, whose infinite power is manifested in every thing that is in the heavens and on the earth.

On Saturday, the 17th of March, Mr. Davy, speaking of the discoveries of

MR. CAVENDISH,

paid the following just tribute to the memory of this distinguished philosopher:

"Of all the philosophers of the present age, Mr. Cavendish was the one who combined, in the highest degree, depth and extent of mathematical knowledge, with delicacy and precision in the methods of experimental research.

"It may be said of him, what perhaps can hardly be said of any other person, that whatever he has done has been perfect at the moment of its production: his processes were all of a finished nature: executed by the hand of a master, they required no correction; and though many of them were performed in the very infancy of chemical philosophy, yet their accuracy and their beauty have remained unimpaired amidst the progress of discovery; and their merits have been illustrated by discussion, and exalted by time.

"In general, the most common motives which induce men to study, are the love of distinction, of glory, or the desire of power, and we have no right to object to motives of this kind; but it ought to be mentioned, in estimating the character of Mr. Cavendish, that his grand stimulus to exertion was evidently the love of truth and of knowledge:—unambitious, unassuming, it was often with difficulty that he was persuaded to bring forward his important discoveries. He disliked notoriety; he was, as it were, fearful of the voice of fame. His labours consequently are recorded with the greatest dignity and simplicity, and in the fewest possible words, without parade or apology; and it

 seemed

seemed as if in publication he was performing, not what was a duty to himself, but, what was a duty to the public.

"His life was devoted to science, and his social hours were passed among a few of his friends, principally members of the Royal Society: he was reserved to strangers, but where he was familiar, his conversation was lively, and full of varied information: upon all subjects of science he was luminous and profound, and in discussion wonderfully acute:—

—"Even to the very last week of his life, when he was nearly 79, he retained his activity of body and all his energy and sagacity of intellect. He was warmly interested in all new subjects of science; and several times, in the course of the last year, witnessed or assisted in some experiments that were carried on in this theatre, or in the laboratory below.

"Since the death of Newton, (if I might be permitted to give an opinion,) England has sustained no scientific loss so great as that of Cavendish; but this loss is less to be regretted, since, like his great predecessor, he died full of years and of glory: his name will be an object of more veneration in future ages than in the present moment:—though it was unknown in the busy scenes of life, or in the popular discussions of the day, it will remain illustrious in the annals of science, which are as unperishable as that nature to which they belong: and it will be an immortal honour to his house, to his age, and to his country."

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting on 3d February, the Rev. Dr. Macknight laid before the society a sketch of the mineralogy of the highlands of Scotland, from the Pass of Leny to Balahelish. The general rock in this tract is mica slate, with its usual subordinate beds, such as, of granular limestone, hornblende slate, &c. It contains also, in some districts, beds and veins of leadglance, and indications of ironglance. Beyond Zyndrum, the mica slate approaches to gneiss, till we pass Inverouran, where sienite appears. In the neighbourhood of King's House, newer granite, feldspar, porphyry and hornstone are found; and the adjacent country, as might be expected from the decomposition of these rocks, presents, for many miles, an unusual aspect of bleakness and sterility. Glencoe, which is singularly interesting, both in a picturesque and in a mineralogical point of view, consists of hornstone and compact feldspar, in beds subordinate to the primitive rocks, and capped with porphyry. At the bottom of Glencoe, mica slate again appears, and is covered

covered with the formation of clay-slate, which affords the well-known quarries of Balahelish. Thus, according to Dr. Macknight, it appears, that the relative positions of the great formations which occur in the highlands of Scotland, correspond to the principles of the geognosy of Werner.

At the same meeting, Professor Jameson read some observations on the universality of rock and metalliferous formations, preliminary to a short account of some specimens of a particular formation of lead-ore found within fifteen miles of Dunkeld in Perthshire. The formation appeared to be almost the same with that which occurs at Strontian in Argyleshire ; and it is therefore possible that it may be a source of wealth to the proprietor.

At this meeting also the secretary read some new and interesting observations on the natural history of the common Greenland whale, by Mr. Willian Scoresby, junior, of Whitby ; and exhibited a correct drawing of that animal by the same gentleman, differing materially from the figures hitherto published.

HACKNEY LITERARY AND PHILOSOPHICAL SOCIETY.

We have great pleasure in announcing the formation of institutions calculated to diffuse useful knowledge. Of this description is the Hackney Society. It consists of two classes. First, Ordinary members, who contribute to the funds, enjoy the use of the books, &c. Secondly, Honorary, consisting of such gentlemen whose association may reflect honour on the society, and whose opinion of the labours of its members may be such as to impress them with sentiments of regard for such a mark of the society's respect. Ladies are admissible as members.

The officers of this society consist of a president, two vice-presidents, two secretaries, a treasurer, and six committee, who are to be chosen from the ordinary members by ballot or scroll at every anniversary meeting.

The meetings on Tuesday evenings are to be principally occupied by literary conversations, and reading such papers on scientific or literary subjects as the society may be favoured with.

The subjects for conversation, and books for the library, are to comprehend the mathematics, natural philosophy and history, chemistry, polite literature, antiquities, civil history, biography, questions of general law and policy, commerce, and the arts.

The purchase of philosophical instruments, and patro-

nizing lectures on philosophical subjects, also form a part of the plan of this society.

FRENCH NATIONAL INSTITUTE.

The class of history and ancient literature of the French Institute has proposed the following as the subject of a prize dissertation : "What were the people who inhabited Cisalpine and Transalpine Gaul, at the different epochs of history anterior to A. D. 410?—Determine the position of the capital cities inhabited by these people, and the extent of territory which they occupied.—Trace the successive changes that took place in consequence of the divisions of the Gauls into provinces."—The prize will be a gold medal of the value of 1500 francs. The memoirs may be written in Latin or in French, and must be transmitted to the Secretariat of the Institute at Paris on or before the 1st of April, 1811.

XL. *Intelligence and Miscellaneous Articles.*

Mr. Brown, the botanist who accompanied Capt. Flinders in the late voyage of discovery, has just published the first volume of a work on the plants of New Holland, &c. under the following title : "*Prodromus Floræ Novæ Hollandiæ et Insulæ Van-Diemen*; exhibens characteres plantarum quas annis 1802—1805 per oras utriusque insulæ collegit et descripsit *Robertus Brown*; insertis passim aliis speciebus auctori hucusque cognitis, seu evulgatis, seu ineditis, præsertim *Banksianis*, in primo itinere navarchi *Cook* detectis."

M. Ebel, of Bavaria, has recently published a geological work on the structure of the Alps, which is reported by the continental journalists to contain much novelty, and to coincide entirely with the experiments made by Humboldt. According to Messrs. Ebel and Humboldt, it is not true that granite is the nucleus of the surface of the earth : on the contrary, we find as many strata of granite as of any of the other integrant substances of mountains. These strata of stones in the mountains have been formed by crystallization in the Sea of Chaos, and are found in a great measure on the same line from Savoy to Hungary. According to these ideas, the earth resembles a prism of crystal, the edges of which have been worn away by the flux and reflux of the waters, without the ruins of these points having entirely filled up the hollows made. These ideas are expected to lead

lead to important results; but they will at the same time discourage those who still hope to find the solid nucleus of the earth. The geologists on the Continent now begin to abandon their own system, in order to embrace that of Humboldt and Ebel.

Mr. Parkinson has withdrawn the *Introduction to the Knowledge of Fossils*, announced at the end of his first volume of *Organic Remains of a former World*, considering its publication as entirely superseded by Mr. Martin's excellent systematic *Outlines* of the same subject. The *third volume* of *Organic Remains* is in considerable forwardness.

In the month of October last, the viceroy of Italy and his consort visited the ruins of the ancient Pompeia, accompanied by chevalier Arditi, superintendant of the Royal Neapolitan Museum. A fresh search having been made for antiquities a few days before by order of their majesties, M. Arditi presented on the above occasion several pieces of ancient pitch, a vessel full of wheat, a piece of coral, several beautiful paintings, and a lamp of baked earth in the form of a leaf, and bearing a Latin inscription. This lamp was covered with a very fine varnish or vitrification, which gave it a silvery or pearly appearance. It seems to be a mistake therefore of some authors, when they inform us that this vitrification was not invented until the fifteenth century by Luc de la Rubria, a Florentine sculptor.

Their majesties having expressed a desire to have some of the ruins dug up under their own inspection, the workmen had the good fortune to find several pieces of money of various denominations: a quantity of bronzes, among which was a very fine vase, and an urn for wine: some articles formed of bones; a great quantity of glasses of various dimensions and shapes; and in particular a great number of vases improperly called Etruscan, on which were Latin inscriptions. On the same occasion, their majesties found some works in marble, and in particular some comic masks: a few small but elegant altars, adorned with bas-reliefs and weights marked with cyphers in the upper part.

Hitherto only a single subterranean apartment had been discovered at Pompeia, improperly called a cantino, but which ought rather to have been named crypto-portico: in the recent diggings one was discovered consisting of several stories. It is remarkable for having a pipe or tube of stucco placed in a corner, and intended for the conveyance of

smoke.

smoke. This discovery seems to set at rest the question so long agitated by the learned; namely: whether the ancients were acquainted with the use of vents or chimneys for carrying off smoke? In the same apartments were also found several pieces of marble and alabaster, valuable on account of the bas-reliefs and inscriptions with which they are adorned.

Their majesties afterwards proceeded into a *triclinium* or dining apartment recently discovered. The walls of this magnificent saloon are covered with paintings of the most exquisite taste, and representing fishes, birds, and game of all kinds. Here there are three couches of mason work in perfect preservation, being the places in which the ancients rested during their meals. Adjoining the three beds, there still exists a marble foot, which must have served as a support for the table on which the dishes were placed.

His majesty on quitting the ruins expressed a most ardent desire that the exertions made to expose them to public view should be continued, and has since issued the necessary orders to his ministers of finance.

The following account of the present state of the universities and other seminaries of education in the new kingdom of Westphalia, is published in the foreign journals. The universities of Halle, Gottingen, Helmstadt, Marbourg and Rinteln, contain in all 1207 students. There are, also, 52 gymnasia or classical schools in the kingdom, at which are educated 6,851 children: the inferior schools, at which reading, writing, and arithmetic are taught, amount to 3,600, and are frequented by 253,338 children of both sexes. In each of the two great cities of Brunswick and Magdeburg there are 35 public institutions for every branch of education, besides private seminaries. In the public schools the hours of teaching are so arranged, that the children who attend them are generally able to earn their livelihood at the intervals. In the above two cities alone 900 scholars are instructed in the sciences. In short, on a moderate computation there is a teacher for every 50 children throughout the kingdom.

The university of Jena is also described as being in a very flourishing condition. The number of students, which in 1807 scarcely exceeded 100, is now quadrupled. The Mineralogical Society established at Jena a few years ago is in great repute. In September last this society held its anniversary

versary, at which the celebrated M. Goethe was present. This ingenious author now devotes the whole of his time to the study of natural philosophy.

A society for the education of the blind has been lately established at Zurich in Switzerland. The number of pupils is at present 50.: and what is singular, the chief master, M. Funke, is blind. He is described as an excellent teacher and an ingenious mechanic.

The calamities experienced at different times in Switzerland from the sudden rolling down of huge fragments of rock and other component parts of the mountains in the Grisons, have suggested to the government the propriety of employing M. Escher, a geologist of Zurich, to survey that district. He has accordingly published the result of his inquiries, and it appears that the valley of Nolla behind the village of Thusis, and the valley of Plesner behind the town of Caire in the Grisons, are threatened with the visitation of avalanches, which can only be averted by the prompt adoption of the measures of precaution which he has suggested.

The following short article on volcanos appears in a recent French journal :—" It has been observed that in the year 1806, at the moment of the dreadful eruption of mount Vesuvius, all the other volcanos with which we are acquainted vomited an increased quantity of flames. Ætna spread terror throughout Sicily, and covered Calabria with lava. Hecla in Iceland was as violent as ever, and the Peak of Teneriffe threw out red hot stones. Volcanos which were thought to be extinguished awoke with new fury.

" The communication of volcanos with each other is not doubtful, But are we acquainted with the conductor of the electrical fluid ?—Is this communication effected by subterranean passages, or by the medium of the atmosphere ? How does it happen that the above unusual phænomena take place at the same moment ?"

CAUTION TO APOTHECARIES AND DRUGGISTS.

For the subjoined information, which we consider ourselves imperiously called upon to circulate as widely as possible, we are indebted to a most respectable manufacturing chemist, Luke Howard, esq., of Plaistow.

A very large quantity of *glass of lead* has, by some means, found its way into the London market, as *glass of antimony*. This criminal *imposition* is sure to be detected, in the operation

ration to which the glass of antimony is chiefly applied, the making of emetic tartar; but it is highly needful, for the sake of the consumers of smaller quantities, as in the *vitrum ceratum*, and *vinum antimonii*, that the following *distinctive characters* of the two be extensively circulated, in order that those, who may have bought the article within twelve or eighteen months past, may assure themselves of its being genuine. *The public health, and even the lives of some patients, may be considered as at stake on the occasion.*

Glass of *antimony* has a rich brown or reddish colour, with the usual transparency of coloured glasses. The glass of *lead* in question is of a deeper and duller colour against the light, is much less transparent, and even, in some samples, quite opake.

The specific gravity of the *true* never exceeds 4·95, that of the *spurious*, or *lead* glass, is 6·95 : or, in round numbers, their comparative weights are as 5 to 7.

Let twenty grains be rubbed fine in a glass mortar, adding half an ounce of good muriatic acid. The *true* dissolves, with an hepatic smell, the solution is turbid, but has no sediment. The *spurious* turns the acid yellow, giving out an oxymuriatic odour, and leaves much sediment.

Let a little of each solution be separately dropt into water. The *true* deposits oxide of antimony, in a copious white coagulum ; or, if the water has been previously tinged with sulphuret of ammonia, in a fine *orange* precipitate—The *spurious* gives no precipitate in water, and in the other liquid, one of a dark brown or olive colour.

A solution of the *spurious* in distilled vinegar has a sweet taste, together with the other properties of acetate of lead.

A very small mixture of the spurious may be detected, by its debasing, more or less, the bright orange colour of the precipitate, thrown down by sulphuret of ammonia from the solution in any acid.

The samples of the *spurious*, hitherto detected, are of a much thicker and clumsier cast than the *genuine* ; but the appearance is not to be trusted, and no specimen should be allowed to pass, without a trial either of the specific gravity, or chemical properties.

John Morison, living at 145, Holborn Bars, who had the misfortune to lose both arms by the discharge of a cannon, has invented a curious set of instruments, so well adapted for almost every purpose of life, that the want of those limbs to him is wonderfully supplied ; and he undertakes to make artificial legs, arms, and instruments, on a new

new principle. The Society for the Encouragement of Arts, Manufactures, and Commerce, being convinced of their great utility in affording help and comfort to unfortunate persons in his situation, lately voted him their silver medal and 40 guineas for his ingenuity.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Stephenson, of Mortimer Street, Cavendish Square, in the county of Middlesex, plumber, for a machine for filtering and purifying of water.—Feb. 27, 1810.

To John Justice, of Dundee, in North Britain, ironmonger, for his improvements in the construction of stove-grates calculated to prevent or cure smoky chimneys, and possessing other advantages over the stove-grates in common use.—March 6.

To Thomas Scott, of Holborn, in the county of Middlesex, musical instrument maker, for his improved German flute, clarionette and oboe.—March 12.

To Thomas Robinson, of Roberts-Bridge, in the parish of Salehurst, in the county of Sussex, brewer, for his mashing machine.—March 12.

To John Kent, of Southampton, architect, for his new and expeditious method of moving all kinds of goods or materials to high buildings or from deep places.—Mar. 12.

To Thomas Grant, of Bideford, in the county of Devon, esq., for his method of making paint or varnish from a new discovered fossil, which will be of great public utility in painting of ships and in various manufactories.—Mar. 22.

To Michael Shannon, of Berwick Street, in the county of Middlesex, architect, for certain improvements in the art of brewing, which were communicated to him by a learned foreigner since deceased.—March 22.

To Johann George Deyerlein, of Long Acre, in the county of Middlesex, tool-maker, in consequence of certain inventions by himself, and of communications made to him by a native of Germany, for a machine, new principle, or method of making bricks and tiles, and also by means thereof, and of clay, loam, or similar materials to those commonly used in potteries, to make all sorts of mouldings, beads, tubes, gutters, channels or cylinders to convey water, smoke, or any fluid or soft substance.—March 22.

To John Gregory, of Islington, in the county of Middlesex, builder, for his new method of tunning or cleansing ales and beers into casks.—March 22.

Rain

Rain Table, by the Rev. J. Blanchard, *of Nottingham.*

1809.	Chichester.	London.	Chatsworth, Derbyshire.	Horncastle, Lincolnshire.	Ferriby, Kingston-upon-Hull.	Heath, near Wakefield, Yorkshire.	Manchester.	Lancaster.	Dalton, Lancashire.	Kendal.	Middleshaw, near Kendal.	Nottingham.
Jan.	8·44	2·91	5·22	3·50	1·57	3·98	2·67	4·66	6·58	5·27	4·48	1·80
Feb.	4·31	1·86	3·29	2·59	2·94	2·58	1·96	3·11	4·53	4·28	5·74	1·69
Mar.	0·00	0·94	0·44	0·82	0·48	0·43	0·35	0·53	1·13	0·72	0·62	0·75
April	3·95	3·46	1·70	2·10	3·05	2·11	0·96	1·59	2·20	1·69	1·50	2·15
May	1·07	0·86	1·83	1·59	0·45	2·96	3·42	3·39	3·85	4·55	2·66	1·80
June	2·38	1·20	2·06	2·24	3·24	2·01	2·45	3·10	4·26	3·24	3·79	2·45
July	3·45	3·58	2·00	2·87	2·38	2·28	1·79	4·00	3·45	3·28	3·08	1·44
Aug.	3·70	2·64	4·38	4·53	5·88	4·61	3·85	6·12	7·25	9·73	9.59	4·50
Sept.	3·34	2·90	4·13	3·90	3·10	4·29	4·22	4·75	5·57	6·36	6·11	3.13
Oct.	0·60	0·22	0·28	0·75	0·56	1·41	0·61	0·87	1·66	1·16	1·10	0·31
Nov.	1·30	1·38	1·91	1·70	1·90	2·25	2·14	3·87	2·80	4·11	2·56	1·18
Dec.	5·53	3·00	2·67	1·79	2·42	2·74	4·68	5·74	7·08	8·02	8·85	1·81
	38·07	24·95	29·91	28·38	27·97	31·65	29·10	41·73	50·36	52·41	50·08	23·01

Meteorological Table, by Dr. Clarke, *of Nottingham.*

Thermometer.				Barometer.				Weather.		Winds.			
Maximum.	Minimum.	Medium.	Greatest Range in Twenty-four Hours.	Maximum.	Minimum.	Medium.	Greatest Range in Twenty-four Hours.	Fair.	Wet.	N. and N.E.	E. and S.E.	S. and S.W.	W. and N.W.
53	17	35·29	14	30·05	28·65	29·44	1·13	18	13	14	18	9	5
54	20	43·00	14	30·33	28·68	29·62	0·74	18	10	2	3	33	16
62	30	44·00	10	30·38	29·00	29·99	0·40	26	5	22	6	9	15
56	28	42·76	13	30·36	28·97	29·73	0·54	13	17	16	1	14	18
77	38	57·61	9	30·26	29·23	29·84	0·43	24	7	10	13	19	13
74	45	57·71	18	30·45	29·27	29·84	0·62	21	9	11	7	15	14
78	46	59·64	10	30·12	29·39	29·38	0·24	17	14	24	4	10	18
76	48	60·69	8	29·97	29·23	29·64	0·45	12	19	3	8	31	8
72	34	50·46	12	29·87	29·05	29·46	0·63	25	5	2	5	11	17
67	30	52·00	10	30·25	29·77	30·09	0·38	25	6	13	6	17	4
54	26	42·10	11	30·41	29·03	29·89	0·77	18	12	12	2	6	22
53	23	40·12	13	30·00	28.25	29·45	0·90	17	14	1	6	23	15
		48·78				29·74		234	131	130	79	197	165

ANNUAL RESULTS.

THERMOMETER.	WIND.
Highest Observation, July 27th	78°S.
Lowest Observation, January 22d	17°N.
Greatest Variation, in twenty-four hours, June 1st-2d	18°
Annual Mean	48·78

BAROMETER.	WIND.
Highest Observation, June 25th	30·45NE.
Lowest Observation, December 17th	28·25W.
Greatest Variation in twenty-four hours, January 30th-31st	1·13
Annual Mean	29·74

RAIN.	INCHES.
Greatest Quantity in August	4·50
Smallest ditto in October	·31
Total Quantity for the Year	23·01

WEATHER.	DAYS.	WIND.	TIMES.
Fair	234	N. & NE.	130
Wet	131	E. & SE.	79
		S. & SW.	197
	365	W. & NW	165
			571

REMARKS.—On the 22d of January, the Thermometer, within two miles of Nottingham, stood at 14°. April 19th, Snow had fallen to the depth of one foot. May 2d, Snow fell this morning. August 6th, the Rain that fell from 9 A.M. to 5 P.M. amounted to 1·72. Loud peals of thunder at noon, increased at 4 P.M., when the lightning became extremely vivid—the thunder tremendous—the rain descending in torrents, and continuing so most part of the night. December 17th, the Barometer at 11 P.M. stood at 28·25f. The following are comparative observations on the fall of the mercury:—

BAROMETER.	*Keswick.*	*Kendal.*	*London.*	*Nottingham.*
January 1788	28·35	28·38	28·89	
January 1789	28·09	28·12	28·58	
December 1809				28·25

N. B. The Barometer is firmly fixed to a standard wall, over a stair-case, on a level of 130 feet above the sea. The Pluviameter is placed in a garden, on an elevation of 140 feet from the level of the sea.

Quantity of Rain in 1808, *at the following Places in Scotland:*

1808.	*Dalkeith.* Inches.	*Bothwell Castle.* Inches.	*Glasgow.* Inches.	*Gordon Castle.* Inches.	*Largs.* Inches.
January	1·29	1·278	1·246	1·27	
February	1·905	1·461	·778	1·62	
March	·452	1·26	·082	·71	
April	2·805	1·923	1·535	2·97	
May	2·22	1·887	1·371	·78	
June	2·288	1·579	1·814	1·62	
July	4·12	1·83	3·118	2·91	
August	3·451	5·827	5·597	2·92	
September	2·651	·655	·616	5·31	1·452
October	2·393	3·848	2·171	3·21	8·033
November	1·48	1·993	2·135	1·68	3·664
December	2·44	1·507	1·342	2·21	5·488
	27·995	24·598	21·795	27·21	18·637

Meteorological

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For March 1810.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 26	38	47°	47°	29·89	23	Fair
27	47	53	44	·65	19	Cloudy
28	40	52	48	·99	32	Fair
March 1	47	53	47	·78	16	Cloudy
2	48	54	49	·69	10	Small rain
3	49	51	41	·55	0	Rain
4	41	50	40	·36	18	Cloudy
5	40	45	39	·16	19	Cloudy
6	35	38	41	28·95	0	Snow
7	40	45	42	·81	0	Rain
8	45	49	46	·99	15	Cloudy
9	52	54	49	29·28	10	Stormy
10	50	54	50	·76	36	Fair
11	50	54	50	·90	30	Cloudy
12	52	52	46	·75	10	Stormy
13	40	42	40	30·00	32	Cloudy
14	39	41	38	30·00	25	Cloudy
15	38	38	35	29·64	20	Cloudy
16	35	39	34	·55	10	Cloudy
17	35	40	33	·65	30	Fair
18	32	44	36	·90	22	Fair
19	29	44	43	30·00	19	Fair
20	34	50	40	29·95	33	Fair
21	35	49	41	·65	21	Cloudy
22	34	42	34	·98	19	Fair
23	32	49	36	·72	34	Fair
24	33	47	37	·72	27	Fair
25	32	42	37	·81	25	Fair
26	35	42	39	·85	27	Fair

N.B. The Barometer's height is taken at one o'clock.

XLI. *On the Torpidity of Animals.* By BENJAMIN SMITH BARTON, *of Philadelphia, M.D.*

To Mr. Tilloch.

SIR, I LATELY purchased, and have just finished the reading of, " An Essay on the Torpidity of Animals, by Henry Reeve, M.D." The work has afforded me much amusement, and some instruction; and may, doubtless, be read with great satisfaction and advantage by the younger class of naturalists. It is, however, I think, less replete with new facts and experiments, and with original and enlarged views of the nature and phænomena of TORPID LIFE, than might have been expected, considering the respectable author's opportunities of acquiring information, and the length of time that he has had the subject under his consideration.

Having myself, for several years, been engaged in inquiries relative to the same subject, in various classes of animals, but especially in the Mammalia, the Birds, and the Reptilia (*Amphibia* of Linnæus), I hope to be able, at no very distant period, to publish the full result of my researches and experiments. I shall then, with that candour, which, I trust, will never forsake me in my inquiries as a naturalist, point out some of the errors (as I now conceive them to be) of Dr. Reeve's work; and, in particular, I shall state, at length, the facts, the actual experiments, and the observations, which compel me to differ from him on some very material questions. At present, I have no other object in view, than to draw your attention, and that of your philosophical readers, to that part of Dr. Reeve's *Essay* which relates to the real or supposed torpidity of birds. This part of his subject, the intelligent author does not seem to have examined with his accustomed ability.

In treating of the " Migration of Birds," Dr. Reeve has the following words: " Here a curious question arises respecting the disappearance of birds. It is singular that this subject should still admit of doubt, when it seems so easy to be decided; yet every month we see queries and answers about the migration of swallows, and every year our curiosity is tempted to be amused with marvellous histories of a party of these birds diving under water in some remote quarter of America. No species of birds, except the swallow, the cuckow, and the woodcock, have been supposed to remain torpid during the winter months. And what is the evidence in favour of so strange and mon-

strous a supposition? Nothing but the most vague testimonies, and histories repugnant to reason and experience*."

It appears somewhat surprising to me, that an author who has so long had the subject of the torpidity of animals under his consideration, should have hazarded the assertion contained in the preceding paragraph. Dr. Reeve has, certainly, read of other birds, besides the swallow, the cuckow, and the woodcock, which are said to have been found in a torpid state. And ought he not to have *mentioned* these birds?

In my *Fragments of the Natural History of Pennsylvania*, which Dr. Reeve, if I do not mistake, has seen, for he has referred to the work in his Inaugural Dissertation published in 1803, I have mentioned the common humming-bird (*trochilus colubris*) as one of those American birds which do occasionally become torpid. I have particular reasons for quoting the passage, as it occurs in the *Fragments*. "I have not been able to learn, that the humming-bird winters in any, not even in the warmest, parts of the United States. I cannot hesitate to consider it as a bird of passage. A gentleman, however, whose name I do not recollect, wrote a little paper to prove, that these birds continue with us all the winter: why? because one of them was, one frosty day, in the month of October, found a good deal benumbed in a church, in some part of New England, I think in Connecticut†."

In the same work, speaking of the *caprimulgus virginianus*, or whip-poor-Will of the Americans, I have said: "I have been informed, that some of these birds have been found in a torpid state, in hollow trees, in New-Jersey. But I cannot entirely depend upon the fact; and I have little hesitation in saying, that this bird, as well as the swallows, to which it is allied, is a bird of passage‡."

Here, then, there are two American birds, besides those enumerated by Dr. Reeve, which are supposed, by some persons, to become torpid in the winter season. Nor do these complete the list. It is the opinion of many well-informed persons, in the United States (but I by no means vouch for the verity of the story), that the Virginia corn-crake, or rail (*rallus virginianus*), becomes torpid, and remains among the mud and grasses of our meadows, &c., during the winter-season. It is asserted, by many other

* An Essay, &c. section ii. pages 39, 40.

† Fragments of the Natural History of Pennsylvania, Part First. Appendix I. pages 18 and 19. Philadelphia, 1799.

‡ Fragments, &c. Appendix I. page 18.

persons,

persons, that whole flocks of the Carolina parrot, or parakeet, (*psittacus carolinensis,*) continue in a torpid state, in the hollows of trees, in the state of North Carolina, and in some other parts of the American Union. *I believe entire dependence may be placed upon this statement*; though it would not be difficult to show, that these birds are often seen abroad, and pretty active, when the ground is whitened by snow. I could mention not a few other birds, the torpid state of which has been spoken of by naturalists and others; and these birds I shall mention in my "Facts, Experiments, and Observations, relative to the Torpidity of Animals."

But "what" (says Dr. Reeve) "is the evidence in favour of so strange and monstrous a supposition? Nothing but the most vague testimonies, and histories repugnant to reason and experience."

This, surely, is not the proper language to be employed in the investigation and discussion of physiological questions. Authorities are facts in natural, as well as in civil, history. And in favour of the torpidity of some of the birds which I have mentioned, the authorities are, sometimes at least, highly respectable: nor are they few in number. In regard to the swallows, I shall say but little at present. I have, at this time, in the press, a memoir on the migration and torpidity of these birds. I am confident that I shall be able to convince every candid philosopher, that great numbers of swallows, of different species, do *occasionally* pass into a state of torpidity, more or less profound, not merely "in some remote quarter of America," but in the vicinity of our capital cities, where there are some men of genuine observation and inquiry, and who are as little propense to believe the marvellous in natural history, as any philosophers elsewhere.

I do not suppose, that *all* the swallows of North America become torpid. It is my present opinion, and it was my opinion when I published the "Fragments" in 1799, that the swallows, *in general*, are migratory birds*. But subsequent and very extensive inquiries have convinced me, that the instances of torpid swallows are much more frequent than I formerly supposed they were; and that there are two species of the genus *Hirundo*, which are peculiarly disposed to pass the brumal season in the cavities of rocks, in the hollows of trees, and in other similar situations,

* See Fragments, &c. Appendix I. page 16. See, also, Introduction to this work, pages xii and xiii. § xxiv, xxv, xxvi.

ations, where they have often been found in a *soporose* state. These species are the *hirundo riparia*, or sand-swallow, commonly called, in the United States, bank-swallow and bank-martin; and the *hirundo pelasgia*, or aculeated swallow, which we call chimney-bird and chimney-swallow. *There is no fact in ornithology better established, than the fact of the occasional torpidity of these two species of Hirundo.*

I say nothing of the torpidity of swallows "under water." But I do not wholly deny *this* fact. And I take much pleasure in referring Dr. Reeve to a short paper, in the *Transactions of the American Philosophical Society*, vol. vi. part i., relative to the discovery of a torpid swallow under a quantity of mud and leaves. The author of that paper was a most worthy and respectable man; and a man so religiously attached to truth, that I believe him to have been incapable of uttering a falsehood. He was, moreover, a man of nice observation, and of a philosophical turn of mind.

I do not wish to urge this part of the swallow's history any further. I have nothing to say in support of the "swallow song." But when, in page 44, Dr. Reeve asserts, that no swallows "were ever found in all the rivers and lakes of England, Wales, Ireland, Scotland, or Switzerland, although fishermen are constantly employed on these their supposed hiding-places," does he mean to say, that it has never been asserted by any of his countrymen, that swallows have been found torpid, under water, *in England?* Swallows are said to have been found torpid "in the river Thames;" and the fact seems to have been credited by some illustrious Englishmen in the 17th century; and among others, if I do not mistake, by the immortal William Harvey *.

But I will take my leave of the swallows.—Since I published my *Fragments*, I have obtained much information relative to the torpidity of the humming-bird. I have hinted at this subject, and have, indeed, most pointedly admitted the fact, in my letter to Mons. la Cepede, published in your Philosophical Magazine. I am now fully persuaded, that

* In Dr. Birch's *History of the Royal Society*, vol. iv., there are some curious notices about swallows. The following may not be deemed wholly unworthy of Dr. Reeve's attention. "Sir John Hoskyns proposed, that it might be duly examined, what becomes of the swallows, and in what state they are during the winter. In answer to which Mr. Henshaw affirmed, that the chancellor of Denmark told him, as an undoubted truth, that in Iceland, there had been taken out of the ice swallows, which being afterwards brought into a warm stove recovered and flew about the room."—Mr. Henshaw observed, "that he had an account like the former concerning swallows from our watermen, viz. that they have found them in the river Thames;

that instances of the torpidity of the *trochilus* are by no means uncommon in the United States: and I regret my having treated with so little respect, the opinion of the Connecticut gentleman already alluded to. It is certain, at least, that the *trochilus*, like the generality of the swallows, is very impatient of cold; and that it sometimes, even in our houses, very suddenly passes into a profound slumber, from which, however, it awakes, to enjoy all the privileges of its life.—I say this is *certain*. And this, so far as his sentiments may be collected from his *Essay*, is more than Dr. Reeve is willing to admit of *any* species in the great class of Birds.

The fact of the torpidity of the *trochilus* was not unknown above two centuries ago. It is related by the Spanish historians Herrera, Ximenes, and several others, though it must be confessed that these writers have mixed with the truth, some *fable*. I have lately conversed with an intelligent gentleman, who was born, and has long resided, in the kingdom of Mexico. He assures me, that the fact of the torpidity of the *trochilus* is known to every one in that country, and in the adjacent provinces. He added, that he had himself seen one of these little birds in its brumal sleep, in a tree.—I shall discuss this subject at length, and shall illustrate it by actual experiments, in my work on the torpid state of animals, to which I have already alluded. In the mean while, I flatter myself that the following lines, a part of which immediately relates to the *somnus* of the *trochilus*, will not be wholly unacceptable to some of your readers. The author is Raphael Landivar, a native of Guatimala; and his poem, entitled *Rusticatio Mexicana*, in fifteen books, besides an Appendix, in verse also, deserves to be much better known than it appears to be. It is, indeed, well worthy of an English translation; and I sincerely wish that the elegant Mr Sotheby, whose translation of the *Georgics* of Virgil has so deservedly procured him a high reputation, could be induced to

Thames; and that towards the end of the year they assemble in great numbers on the little islands of the river, and then submerge themselves in the water."—"Upon reading the minutes of the last meeting, Mr. Henshaw remarked, that Dr. Harvey had considered the state of swallows in the winter, and had dissected some of them, which had been found under water, and could not observe that there was either warmth or motion in them."—"Mr. Chetwynd, of Ingstree, being present (at a meeting of the Royal Society), observed, that during the time that the swallows are laid up for the winter, they moult, and return in the spring with all new feathers." The History of the Royal Society of London, &c. &c. By Thomas Birch, D.D. Secretary to the Royal Society, vol. iv. pages 533, 534. 537.

undertake

undertake the task. My copy of Landivar's work, which is, I believe, a very rare one, would be at his service. The public pulse might be tried, by the publication of a version of one or two of the books.

In his 13th book the author treats of Birds. And here it is that he speaks of the humming-bird, its manners, its sleep, &c.

"Nil tamen exiguo novit præstantius orbis
Colibrio dulcis spoliato murmure vocis*,
Sed claro tenues pennâ radiante per artus.
Exiguum corpus, forsan non pollice majus,
(Quod rostro natura parens munivit acuto
Atque artus ferme totos æquante volucris.)
Induit aurato viridantes lumine plumas,
Et varios miscet tractos a sole colores.
Ille volat rapidum Zephyrum superante volatu,
Et raucum pennâ tollit stridente susurrum.
Roscida si vero fragranti educere flore
Mella velit rostro, viresque reducere membris,
(Quippe aliâ quacumque negat se pascere mensâ)
Sistitur in medio concussis aëre pennis,
Nectareum donec tereti trahat ore liquorem.
Ast adeo prompte subtiles concutit alas,
Ut vigiles fugiant oculos, ludantque citatæ;
Suspensamque putes volucrem super æthera filo.
Sin autem sylvis borealis bruma propinquet,
Plusque vagus solito frigescat Jupiter imbre,
Frigida præcipiti linquit *Colibrius* arva
Nostra fugâ, linquitque levi viridaria pennâ,
Et longùm montis nigris absconditus umbris
Indulget placido, ceu Progne arguta, sopori,
Dum luces Aries stellatis noctibus æquet,
Verque novum pratis antiquum reddat hororem."

Rusticatio Mexicana, lib. xiii. v. 217—242.

All this, Dr. Reeve will perhaps say, may do very well in poetry: but something more positive on the subject of the "placidus sopor" of the colibri is required. Some facts, and therefore something more positive, I have already mentioned: and many additional facts, with experiments, I promise to give in another place. At present I will only add, that Mr. Landivar mentions the torpidity of the humming-bird, not as a fable, but as an established truth. For in the short *Monitum* prefixed to his interesting work, he

* "Avicula hæc *Colibri* in America Meridionali, in Septentrionali vero *Chupa-mirto* dicitur." Note by Landivar.

he says, "In hoc autem opusculo nullus erit fictioni locus, eam si excipias, quæ ad lacum Mexicanum canentes poetas inducit. Quæ vidi refero, quæque mihi testes oculati, cæteroquin veracissimi, retulere. Præterea curæ mihi fuit oculatorum testium auctoritate subscripta, quæ rariora sunt, confirmare." I am, sir, with much respect,

Your obedient servant, &c.,

Philadelphia, Nov. 1, 1809. BENJAMIN SMITH BARTON.

XLII. *Description of a Hydro-pneumatic Table for collecting and transferring Gases, and for experimenting on permanently elastic Fluids. By* FREDRICK ACCUM, *Operative Chemist, Lecturer on Chemistry and on Mineralogy and Pharmacy, M.R.I.A., F.L.S., &c.**

THERE is but one sure road to arrive at truth in all departments of experimental knowledge, namely, to consult nature herself by operative experiment; and this becomes the more necessary as the subjects to be studied are more complicated and extended.

In mathematics, the propositions necessary to be known for the acquisition of knowledge, are self-evident, and admitted as soon as announced, and from their ready admission the student is led to the explanation of the most complicated truth. In the system of chemical science it is otherwise. This branch of inquiry, it may be said, opens with a detail of various processes which are by no means self-evident, nor easily to be repeated by those who have not yet a claim to the title of a chemist.

To awaken the ardour of chemical research, as well as to give facility to the practical acquisition of knowledge at the least possible cost, and in the most advantageous manner, must therefore be an object of some importance to the advancement of science; for to the abridgement of labour, and the superior aid of modern instruments of research, does the science of chemistry owe its rapid strides towards perfection.

It is well known that before the invention of the thermometer, men were accustomed to judge of the different degrees of heat and cold, by the sensations produced on their organs of feeling, and the estimates must have been often highly exaggerated, and always vague and fallacious. It must be acknowledged that a number of valuable facts

* Communicated by Mr. Accum.

would still be wanting, chiefly with regard to our knowledge of the constitution of mineral bodies, if Bergman had not pointed out the effects of the blowpipe, an instrument of the greatest value to the mineralogical chemist.

What a deal of trouble and expense, as well as time, (that most inestimable of all the *desiderata* of experimental research,) is saved by the form which Nicholson has given to the gravimeter, an instrument which renders the modes of ascertaining the specific gravities of solids and fluids, at once easy, expeditious, and accurate, to the fifth figure of the decimal, water being taken as unity *.

What a vast field of inquiry has been opened to the chemical philosopher by the very simple modification of the air-thermometer of Leslie,—an instrument of uncommon delicacy, employed by him with the greatest advantage in his important researches concerning the mensuration of the force and density of light! and with what ease, expedition and œconomy are at present performed (even in the closet) those operations which in former times demanded a regular laboratory! The moveable furnace of Dr. Black †, the blast furnace of Aikin, and the lamp furnace of Guiton, are now considered as sufficient for carrying on, in the small way, all those operations of the science, in which either a very intense or a very low heat is necessary. These apparatus alone, it may be said, have banished from the laboratory a number of unwieldy and costly contrivances, which served only to excite embarrassment and confusion. Many tedious processes in the practice of our science have thus been made easy by the help of modern instruments, hazardous ones are become safe, expensive ones cheap, and the means of experimenting have been brought to every door. The cultivators of chemical science who have no access to the laboratory of the operative chemist, are too well acquainted

* It does not appear that any better instrument for finding specific gravities for the use of the chemist need be wished for. The gravimeter of Nicholson is susceptible of correction for the variation of temperature, and the impurity of the water in which it is to be immersed, which for practice is sometimes more convenient. It requires no address in using it, and the price at which the instrument may be purchased is far below that of the most ordinary kind of hydrostatic balance.

† This furnace has been considerably improved by others, since its first introduction into practice. It is very substantial, durable, not liable to be easily injured by external blows, and capable of producing a degree of heat more than sufficient to melt iron. It is perfectly safe in a room; and the thickness of the walls, composed of fire bricks, with which it is lined, prevents the operator from being molested by the heat when the furnace is in action. See "Manual of Analytical Mineralogy, intended to facilitate the practical Analysis of Minerals," by F. Accum, 2d edit. page 38.

with

with the utility of the instruments before named, to render any further observations concerning them necessary.

It is in consequence of such reflections, and the invitations I have received from others, whose judgement I respect, that I take the freedom to lay before the public the annexed sketch of a pneumatic table, which in the routine of my profession I have found extremely useful in operating on gases, which I flatter myself will be found an acquisition among the apparatus to the laboratoty.

The discovery of the gases, and their great importance in the researches of modern chemistry, have occasioned, as is well known, the necessity of some peculiar instruments, by means of which these bodies may be caught, collected, transferred, and submitted to the action of other bodies. Among these the very simple and ingenious reservoir, invented by Dr. Priestley, and named by him the pneumatic trough, is the most indispensable. Several alterations have been proposed in the structure of this vessel, to render it more convenient for use; but these, it may be said, relate either to its form only, or to its neatness and general appearance, and not to its principles, or application and utility, as connected with the operations of pneumatic chemistry.

Fig. 1. (Plate VII.) represents a wooden table three feet six inches high, two feet ten inches long, and one foot eight inches broad. At each end of this table, and at a depth of ten inches from its upper edge, is a moveable board or platform *d d* measuring 18 inches by 16½. These platforms are supported horizontally by swing-brackets *e e*, which may be turned aside to allow the platforms *d d* to fall down between the inner sides of the legs of the table. The brackets *e e* are then concealed from view. The platforms *d d* serve to support table furnaces, retorts, and stands, or other apparatus employed in the production of gases. Fig. 2. is an œconomical lamp furnace with its retort in action, to show the use of the platforms. The upper part of the pneumatic table is surrounded by a broad rim or border *x x* two inches and a half deep, so as to form a shallow tray. It is of rather larger dimensions than the table, projecting over the frame of it about three fourths of an inch. This tray (as I shall call it) is divided into two unequal compartments, namely, a cistern *a a* occupying one end or side, and a stage or shallow plain *b* occupying the other. The cistern *a a* is 16 inches deep, 19 wide, and 16 broad; the stage or plain *b* occupies the remaining part of the tray *b*. When the table is intended to be

be used, the tray *x x x*, together with its cistern *a a*, and stage or plain *b*, is to be filled with water, so as to stand at least three fourths of an inch deep over the shallow plain or stage *b* in every direction. Across the cistern *a a* is a shelf, marked *c* in the drawing; it is five inches broad, and perfectly level with the stage *b*. This shelf slides backwards and forwards horizontally within the cistern *a*, so that it may be placed at any convenient distance from the lamp furnace, fig. 2, or other vessel from which gas proceeds. It is provided on its foremost edge with a row of holes, into which from underneath, broad short-necked funnels are fixed; it serves to support the receivers standing on the shelf *c*, with their open ends turned downwards upon the before-mentioned holes, through which afterwards the gas conveyed by the funnels is made to pass into the jars destined to receive it. As this shelf is level with the stage, and may, by its sliding motion, be brought close to the stage *b* where it terminates in the cistern, the jars placed upon this shelf, when filled with gas, may be slided along without further trouble to a distant part on the adjoining plain *b*, whilst other vessels previously filled with water in the cistern *a a*, and standing also on the plain *b*, may in return instantly be moved on the shelf *c*; and thus the trouble of removing the jars into and out of the cistern is totally avoided.

From this statement it will be seen that the advantages of the pneumatic table are considerable. It enables the operator to fill jars or vessels with the utmost neatness, convenience and expedition. The necessity of transferring them when filled with gas, out of the trough, is avoided, and the danger of suffering part of the gas to escape, as well as the almost unavoidable spilling of water whilst the vessels are removed, is completely guarded against. It enables the operator also to fill jars of almost any size, which cannot be done in the common trough, without rendering it unwieldy and very cumbersome, or without being at least obliged to immerse the jars previously into a deeper trough, and thence conveying them upon a plate or saucer into the reservoir in which they are to be filled with gas. Thus the constant danger of having either too much water, so as to overflow the trough, or too little, so as to admit common air to enter into the jars when the water sinks below the shelf, is effectually remedied. In the common trough the vessels are also exceedingly liable to be overset by the pressure of the water,

water, the height of which rapidly increases when large jars are filled, and require continual removing.

When large quantities of gases are required, as in public lectures, this pneumatic table has been found exceedingly useful. The operator not being restrained for room in the management of his experiments, nor being obliged to transfer the vessels continually in and out of the apparatus, and being enabled to perform his operations within a confined space and without encroaching upon any surrounding furniture, are likewise no small advantages, which entitle it to the attention of those who have no access to the laboratory of the operative chemist. The shelf *ff* is very serviceable as a receptacle for holding whatever utensils may be deemed necessary to be near at hand during the experiment.

Compton Street, Soho, March 12, 1810. FREDRICK ACCUM.

XLIII. *On Salmon-Fry: in Answer to a Correspondent whose Communication appeared in our last Number.* By JOHN CARR, *Esq., of Manchester**.

To Mr. Tilloch.

SIR, A LIBERAL discussion of subjects in British natural history certainly affords a very amusing variety amongst the more abstruse branches of scientific research, which obtain so deserved a preference in your select journal; and it is with this view that I submit my present observations, rather than from a wish to attain any triumph over the brief and illusory remarks on Salmon-leaps, in your Magazine for last month. What I formerly stated, and shall now offer, is the result of my own personal observations in the Tweed, Coquet, Tyne, Eden, Esk, and several other northern rivers which swarm with salmon, and I mention this only as increasing the responsibility for what I shall advance.

Were it really a fact, as contended for by the writer I am opposing, that salmon-fry actually continue upwards of twelve months in the same river where they are spawned; then during the summer months, (when the streams are

* Mr. Carr is respectfully informed that, if we have not misunderstood his allusion, he is mistaken respecting the author of the paper to which he has sent this as an answer. We have therefore suppressed his introductory paragraph.—EDIT.

half

half dried up, and the water transparent,) however diminutive might be their size, such are their incredible multitudes that they could no more remain concealed than the pebbles over which the streams flow; and yet during this season, so favourable to exposure and discovery, I believe not one solitary individual of the preceding spawning season can be met with.

In the Tweed, more than two hundred thousand salmon are annually caught; and these, astonishing as the number may seem, probably bear but a moderate proportion to the whole quantity that enters the river. Every female deposits many thousands of ova; hence it is obvious what myriads of progeny must be the result; and accordingly in the months of March and April the amount of fry in the river fully corresponds with these data; but after the vernal floods, no vestige remains of the true salmon-fry, the whole of the immense body being swept down into the ocean.

The writer in your last Number seems to be unacquainted with the spawning months, which are not the two which he conceives, but the last four months of the year. The fish by no means all spawn at or near the same time, there being considerable diversity in that respect, some commencing and even finishing two or three months before others. This diversity probably has two very different circumstances for its cause,—the different ages of the fish, and the different periods of their entrance into the fresh waters. The weather, and state of the rivers as to floods, have also an evident influence. It is in the months of June and July that the old fish, recovered from the great expenditure of health and substance in the preceding spawning season, first begin to ascend the rivers. What are called fresh fish, indeed, come into the rivers even in December, and in all the following months, but these are fish which have not spawned in the last season. In September the females are all exceedingly distended with ova, and a few of them even begin spawning in that month, more in that following, but November and December are the principal spawning months; and that the ova then deposited are the germs of the same fry which appear so abundantly in the spring, is a truth as well known, and ascertained, as that the fish themselves have heads and tails.

The very rapid growth of the fry I specially noticed, as an extraordinary circumstance; but we shall advance a very short way indeed into natural research, if the character of extraordinary is to beat us back. The average weight of the

the fry in spring will not amount to one ounce; and the writer who has made it necessary for me to offer these observations, admits that the spring fry in the following July and August attain a weight of from 14 to 20 ounces. This latter fact I know to be much greater than stated; and surely the writer who can cavil at the acquisition of the first ounce, and immediately admit its multiple by twenty in little more than the same period, must have very incongruous notions on the subject. His paper was professedly written to exhibit what he calls a more rational and credible account of the matter than I had given; and its own contradiction and improbability are strikingly manifest, as will be seen by contrasting the following two passages: "The fact is, the young fry do not descend the rivers with the old salmon in the spring after they are spawned, for in the October following they are no bigger than a minnow."—"In the months of June and July they (the fry) are about five or six inches in length: *this I know to be fact.*"

These paragraphs contain the horns of a piercing dilemma, and I offer the writer his choice of being gored with that which he may prefer. Either the same fry which were five and six inches long in June and July have, by some strange means, been wasted down to the size of a minnow in the following October, or the fry then no bigger than a minnow continue in the river till the following June and July: and in this latter case, whence have those shoals of fry originated which the same writer sends down to the sea in spring?

The fish which he speaks of in the months of June and July, is an abundant and well-known visitor in our rivers at that season; but it is a perfectly distinct species of tself, and by no means the salmon-fry, with which, however, it has long been, and with the ill-informed it still is, confounded. *It is the samlet.* Few animated productions of nature have had a more contested origin, or been subjected to more wild and extravagant assumptions. Most generally it was considered as a backward and spurious brood of the salmon, that always remained dwarf; never bred; and even all the individuals were absurdly deemed males. Its natural history is now, however, well ascertained, and, excepting in size, it certainly does approximate, in all respects, very nearly to the salmon. Like the latter, it ascends our rivers, from the ocean, in summer, continues in great abundance through the autumn, afterwards spawns, and in the first two months of the year again descends to the

sea.

sea. It probably never exceeds nine, but averages only six inches in length. Though every where the same precise species, its dubious history has assigned it a number of provincial names. In Scotland they are called *pars*, in Cumberland *bremlins*, in Northumberland *wrack-riders* (from their lying on the wrack or river weed), in the Severn *samsons*, and in the Wye *skirlings* or *lasprings*.

Mr. Pennant, whose industrious research after facts has so seldom been equalled, in combating the notion of their being salmon-fry makes the following observations: "The samlet is the least of the trout kind. It is by several imagined to be the fry of the salmon: but our reasons for dissenting from that opinion are these. First, it is well known that the salmon-fry never continue in fresh water the whole year: but, as numerous as they appear on their first escape from the spawn, all vanish on the first vernal flood that happens, which sweeps them into the sea, and scarce leaves one behind. Secondly, the growth of the salmon-fry is so quick and so considerable as suddenly to exceed the bulk of the largest samlet: for example, the fry that have quitted the fresh water, in the spring, not larger than gudgeons, return into it again a foot or more in length. Thirdly, the salmon attain a considerable bulk before they begin to breed: the samlets, on the contrary, are found, male and female (distinguished by the milt and roe), of their common size. Fourthly, they are found in the fresh waters in all times of the year."

These observations of Mr. Pennant I can fully confirm from my own experience, excepting the last, which I must directly contradict; for I believe it would be absolutely impossible to find a single samlet in any of our rivers in the months of March and April, the very period when the waters are teeming with the proper salmon-fry. This little interesting fish, the samlet, in natural habitudes, figure, and even oily fatness, is truly a salmon in miniature; but, nevertheless, it does contain several discriminative marks,—in so much that were a samlet and salmon-fry, of equal size, placed together, the most cursory eye would readily recognise a difference. The samlet is thicker and fuller made than the fry, so much so as to be one third heavier in equal lengths: but what would instantly distinguish the one from the other, are several broad light blue bars crossing the sides from the gills to the tail. These the samlet never is without, nor does the fry ever possess them, though similar blue marks may often be seen on small trout. The back and fins of the fry are also more dusky, and the few red spots

spots along the lateral lines far more dull and obscure than in the samlet. The latter are of all fish one of the easiest prey to the angle. They are incessantly on the feed; and an expert angler with artificial flies, in some of our rivers, will take eight or ten dozen in a few hours.

Besides the salmon and samlet, the bulltrout, seatrout, and whitling, all very distinct species of the same genus, also quit the ocean and ascend our rivers, to spawn, in great numbers, and their fry are frequently mistaken, by incautious observers, for the true salmon-fry; from which, however, they exhibit several specific differences.

I shall now drop the subject, having, I presume, sufficiently shown that my former account of the salmon was strictly correct. Even the writer who has drawn from me the present remarks, will, I trust, by this time have discovered how much more easy it is to derange and embarrass than to elucidate an obscure case.

It is worth observing, that the papers on the breeding of fish, published in the Philosophical Magazine for October and November, very satisfactorily illustrate an obscure fact in the natural history of the trout. It is well and very generally known, that trout, when confined in ponds and lakes, attain a size very far beyond what they ever arrive at in rivers and brooks, and that in these confined situations they never breed. This is well accounted for by the proof that they can only spawn in a swift-running current on gravel, and that there only the spawn will attain animation: and doubtless the fish not being exhausted with breeding, in situations where no sufficient currents exist, is a principal cause of their extraordinary growth. These considerations are very encouraging for stocking large pools with trout-fry procured in the easy way pointed out in the papers I have alluded to, and by which trout of a very superior size may be obtained.

A most singular anomaly in the history of British fishes has lately occurred, by a very splendid and meritorious work, under that title, having issued from the press, wherein two species of our most abundant and interesting fish, the salmon and common eel, are purposely disregarded, under the inadequate excuse of making room for a few scarce, obscure, and immaterial species. My own cursory remarks on the two proscribed species will sufficiently establish, that, to an industrious and intelligent inquirer, they yet offer much of new and interesting matter. The most, I was about to say the only, valuable portions of natural history consist in a knowledge of the singular and various habitudes

tudes and peculiar modes of existence in each distinct class,—a sort of animal biography, wherein the species is personified in the diversified details of the individual: and applying this to the case under consideration, surely the animals which almost daily appear upon our tables have a paramount claim to our consideration, over those which can never extend beyond administering to, certainly a laudable but yet, a mere curiosity. I am, sir,

Your most obedient humble servant,

Princess Street, Manchester, JOHN CARR.
April 6, 1810.

XLIV. *On the supposed Fresh-water Origin of the Gypsum Strata in the Environs of Paris; on the Geological Characters and Relations of the Alum Shales on the Northern Coasts of Yorkshire: and on the Orbicular Blocks of Sienite on Mount-Sorrel Hill, in Leicestershire. By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR, IT has given me great pleasure to find, that the doubts which I ventured to express in pages 134 and 139 of your Philosophical and Geological Magazine, as to the accuracy of Messrs. Cuvier and Brogniart's opinions, on the *fresh-water origin* of the lower part of the series of strata described in the IVth article of their memoir on the basin of Paris*, are in the way of being cleared up, by the researches of Messrs. Desmarets, the elder and younger, and M. Prevost, who have found numerous *sea-shells* in the third or lowest gypsum mass, notwithstanding M. Cuvier and his associate had said (page 50 of your translation) "we are not acquainted with any fossil in this mass, which is the third of the quarries:" what therefore must become of the opinion of M. Lamanon, adopted by them and others, as to the gypsums of Montmartre and the other hillocks of the basin of Paris, having been crystallized in fresh-water lakes? or, thought of the unphilosophical expression used on the occasion, that *one fresh-water shell* therein, was "sufficient to *demonstrate* the truth of the opinion of M. Lamanon?" In a calcareous marl, below what is called the little plaster bed in this mass, the fossils appear numerous, and are, ampullariæ, calyptræ, cerites, cithereæ, cockles, corbulæ, crab's shells and claws, echini spatangus†, glosopetræ,

* Page 49 of the present volume.

† At Grignon lime quarries (p. 118) our authors observed echini cypeastræ.

murices,

murices, solens, tellinæ, turritellæ, vertebræ of fish, volutes, &c. Below the above, three small beds of plaster and some strata of marle occur, without shells; and lower, a bed of calcareous marle, in the midst of which is a plaster or gypsum bed, which contains cerites of the genera petricolum and terebrale; many of which *sea shells* are perfectly similar to those in coarse limestone at Grignon, and other places, not only in marles here, lying between beds of gypsum, but in the gypsum itself, although several naturalists have questioned the reality of such an occurrence. The above particulars I have learnt, from a notice in Mr. Nicholson's Journal of the present month, which contains other interesting particulars. Should M. Desmarets and Prevost have presented any memoir on the above subject, you will not, I hope, fail to translate it at length, on its reaching this country.

In the respectable journal above mentioned, Mr. Richard Winter has given an interesting account of the manufacture of *alum* in the neighbourhood of Whitby, and of the strata whence the raw material or alum-shale for making this useful article is procured; of which geological particulars I beg to present a brief sketch, for the purpose of soliciting further information, through the medium of your Magazine, such that may enable us to fix the place of these strata in the British series.

Alum-shale, a blueish gray argillaceous schistus, occupies the coast of Yorkshire for about ten miles southward, and 18 miles northward of the town of Whitby, and extends inland a great distance; cliffs of this shale appear on the shore which are from 100 to 750 feet high, and on which the sea is continuing to make its inroads. A ferruginous sand-stone (perhaps the same with that of Woburn), iron-stone, and shale, or clay of its decomposition, covers the alum-shale stratum, having an easy dip to the southwest. The sand-stone appears from 4 to 50 or more yards thick, producing ochrey springs at its bottom; it hardens by exposure in buildings: a thin bad seam of coal or jet, (probably bituminized wood,) seldom exceeding two inches in thickness, is sometimes found under or in the lower part of the sand-stone: the slines or length-way joints of the strata of stone and shale range N. and S., and the end or cross joints E. and W.

The alum shale contains several species of *ammonitæ*, abundance of *belemnitæ*, *bones* of animals, *nautili* of two or three species, *shells* of various kinds, *trochitæ*, and *vertebræ* of fishes or animals. It likewise holds nodules of

indurated clay, which contain *native alum*; geodes of iron-stone that contain *naphtha*; pieces of petrified, charred and bituminated *wood*, or *jet*: red *iron-stone*, in strata of a few inches to two feet thick, four or five in number, at about 200 feet from the top of the shale: *calcareous spar*, about half an inch thick, in the vertical slines and end-joints of the shale above mentioned: *whin-dykes* are said also to traverse these strata, and to have charred the coals in their vicinity? The specific gravity of the alum-shale is about 2·48; it decomposes into clay on long exposure: its upper part is richest in sulphur, and produces about one ton of alum from 130 tons of shale, or 00·77 per cent.: the lower part of the shale is very bituminous, hard and slaty; a spontaneous and slow combustion ensues when the alum-shale is mixed and sprinkled with sea-water.

Such are the interesting geological facts, which Mr. Winter has laid before the public, respecting the aluminous shale strata of Whitby; concerning which, it is further desirable to know, the names of the different species of imbedded shells and other organic remains, with reference to published descriptions and plates of them, or drawings and descriptions of such as have not yet been described by naturalists; and an account of the strata that underlay the alum-shale, as well as of those which overlay the ferruginous sand-stone. To ascertain these last particulars, it may be necessary to examine the extremities of the alum-shale on the sea-shore, and to trace its edges or boundaries for some miles inland; carefully distinguishing between the *alluvial* matters, properly so called, consisting of or containing *rounded stones* and broken and heterogeneous mechanical mixtures, and the regular stratified matters, which will somewhere be found in a regular range, covering the ferruginous rock and other strata which Mr. Winter has described; the nature and peculiar fossils of which strata, it will be very desirable to obtain an account of.

In Derbyshire, some are of opinion, that the great or limestone Shale which there covers the mineral limestone and basaltic strata, as shown in Plate II of your thirty-first volume*, (and of which *Mam Tor* is composed,) is the same soil or assemblage of strata, which produces alum at

* And is described by Mr. Whitehurst in his "Inquiry concerning the Earth," 2d edit. p. 183; where however he is wrong in saying, that this shale contains no vegetable impressions, since such occur at Shaw-Engine mine there mentioned, and numerous other places. Mr. Mawe, in his "Mineralogy of Derbyshire," p. 24, has copied this error, and added a still greater one, viz. that this shale "is not stratified."

Whitby

Whitby and the adjacent parts of Yorkshire; and at Shaw-Engine mine in particular, near Eyam N. (where this stratum has been penetrated 360 feet deep to come at the vein of lead ore in the first limestone) alum is said to form a crust on the parts of the shale exposed. I cannot however consider this shale, and that at Whitby, as belonging to the same soil, but believe the latter to correspond with the great or clunch Clay of Bedfordshire, Cambridgeshire, Lincolnshire, &c., underlaying the Woburn Sand and ferruginous sand-stone strata, as hinted above: which clay abounds with selenite and pyrites, and at its exit from the island on the south coast in Dorsetshire, if I mistake not, takes fire on contact with the sea-water, as at Whitby; and whose highly bituminous shales, towards its lower part, have occasioned the useless expenditure of such immense sums of money in search of coals in this and past ages, in all its course through the English counties. At the east end of Bolinbroke town in Lincolnshire, in 1807, I observed these bituminous shale beds, and within a few miles, an expensive boring was at that time making, in search of coal-seams, which existed only in the ideas of the adventurer. Ludi Helmontii of various sizes and shapes, some long and cylindrical in shape, (of which sir Joseph Banks has specimens at his seat at Revesby) with thin shells investing them, which seem to prove their *animal origin*, are found at Bolinbroke, and elsewhere: Quere,—Are ludi found in the Whitby shales? or large anomia gryphus, perforated, or worm-eaten as it is called, near to the sand-stone?

Mr. Winter mentions the Whitby strata, as passing into the interior of Yorkshire and into Lancashire; but is it not more probable, that the ancient alum-works in Lancashire and the western parts of Yorkshire, which he alludes to, were situate on the shale of Castleton Mam Tor, above mentioned? and not on the Whitby shales; since the former contains sulphur sufficient for the production of the sulphuric acid, necessary in the production of alum. An alphabetical list of all the places, exactly describing their situation, where alum has at any time been made in the north of England*, which perhaps Mr. Winter by the assistance of his friends could furnish, would prove of considerable use towards deciding the geological questions above stated, as to the number and relations of the rich alu-

* Benjamin Martin's Natural History of England, vol. ii. p. 229, mentions alum as among the mineral products of Derbyshire; but I found no works, nor heard of any, in my recent examination of that county.

minous soils, in the British series of strata. The Whitby strata I have never had an opportunity of seeing, or any other where alum is manufactured; nor do I recollect what Mr. William Smith has ascertained, relative to the Whitby and other alum-shales. The valuable information which Mr. Winter has already given on this subject, is, as I hope, but the prelude to further details, which would greatly oblige,

Sir, your obedient servant,

JOHN FAREY.

No. 12, Upper Crown Street,
Westminster, April 5, 1810.

P. S. The perusal of an extract in your last Number (p. 222), respecting the orbicular blocks of granite of Talavo and Sartene, and the doubts expressed, whether the latter received their present form by the effects of attrition or decomposition, induce me to state, that in a cursory examination of Charnwood Forest in Leicestershire, in August 1807, previous to entering on my Derbyshire survey, I found on Mount-Sorrel common, on the north and north-west of the windmill, several isolated blocks of sienite, from one to several feet of solid content each, which appeared at first sight like rounded stones, almost half imbedded in the soil; but on a closer examination I found them to be perfectly similar in kind to other blocks nearly cubical, which were lying about on the grass, of which blocks seats are made at the doors of many of the houses in Mount-Sorrel town, which most travellers that way must have observed. Seeing reasons for referring the rounding of the blocks in question to decomposition rather than attrition, I attempted, and succeeded in two or three instances, in turning over these blocks, by which it appeared evident to me, that attrition had had no share in giving them this semi-orbicular form; the bottom in contact with the soil, presenting a perfect face of the cubical form peculiar to these blocks, and no appearance of breaking or other violence: in a further search on this common, I found such a series of these blocks in all stages of their decomposition, as to leave no doubt on my mind on the subject.

In the progress of my subsequent survey, I have found numerous instances of round masses of granite, of very different species apparently, some very large, but none of these boulders admitted of any hesitation in referring their form to attrition, or in considering them as the alluvial ruins of very distant regions. One of these granitic boulders I found in the Buts pasture at Ashover, on the limestone, some in Bretby Park, on the red marle, some on Maccles-

field

field common, Cheshire, on the coal-measures, some in alluvial marle on Werneth-Low Hill in Cheshire, (as mentioned vol. xxxiv. p. 50,) and in vast numbers in the vale of the Goyte from Whaley-bridge downwards, and in its various branches in Derbyshire and Cheshire: in numerous other places, single blocks are found. What I wish to impress on geological observers is, the necessity of stricter attention to alluvial matters, and not hastily to conclude whether round stones are boulders, or were formed by decomposition.—J. F.

XLV. *On Crystallography. By M.* Haüy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 201.]

ON THE NOMENCLATURE OF MINERALS.

Mineralogy and the other natural sciences have been cultivated during a long course of years, without men of science being aware of the great influence possessed by words, the signs of our ideas, in facilitating the acquisition of their ideas themselves. The language of the sciences was not submitted to any fixed rule; the caprice of the nomenclators themselves decided both the choice and the number of the words which composed each denomination; and these words, frequently improper, or even calculated to lead to a fallacious interpretation, presented the double inconvenience of injuring the operation of the memory and of obscuring intellectual perceptions.

At length Linnæus undertook to make natural history speak a rational and methodical language, by reducing every denomination to two words, one of which should be common to the species denominated, with all those which belonged to the same genus, and the other should serve as the distinguishing sign of this species. The example of this illustrious man has been followed by all those who have since cultivated the study of nature with most success; and the authors of modern chemistry have adopted a similar precision in the idiom of that science, in which it is conjoined with a peculiar advantage originating from the very basis of the subject. It consists in this, that here, to name and to define are one and the same thing; and a mere collection of the names, such as fluate of lime, sulphate of barytes, &c. presents an abridged treatise on the science.

We

We have adopted this nomenclature wherever the knowledge we have acquired would admit of it; and amidst a crowd of examples which we could quote, in order to prove how much mineralogy has gained by this adoption, we shall confine ourselves to what is furnished by the word *spar*. Originally several species of minerals which had a lamellous and opal-like (*chatoyant*) texture were united under this head. Thus there were *calcareous spars*, *heavy spars*, *fluor spars*, *shining spars*, &c. At the time when the different bodies designated by this word composed a single genus, as seems to have been the case at the origin of the science, it was the method that was more in fault than the nomenclature, by identifying species essentially different from each other. Subsequently, however, these same bodies, having been better known, were separated from each other and placed in different genera, or even in different classes, and nevertheless they were not allowed to preserve their common denomination of *spar*; and the inevitable alternative occurred, either to parcel out a genus in order to disperse its members, which is contrary to all the principles of the method, or to confound under one and the same name, genera which in other respects had nothing common, which is not less opposite to the principles of a good nomenclature. And as if there was not enough of confusion occasioned by the spars of the ancient mineralogy, the abuse of this word has, as it were, multiplied in modern denominations; and hence we have boracic spars, adamantine spars, &c. The language of modern chemistry, by suppressing the word *spar* in the acidiferous substances, has given a kind of signal-post for extending the same reform to some of the earthy substances which still remained obscured by this erroneous appellation*.

As to the names of these last substances, they ought to be founded, at least for the present, on considerations foreign to the chemical nature of bodies; and it is even to be presumed that we shall not be yet able to refer them to the results of analysis, always supposing that we are not stopped by the prolixity of those which should be applied to substances composed of three or four earths intimately combined with each other. Whatever may be the case, names are wanted which could serve during an indefinite time, and this was one reason for also making in this part of

* We have preserved this word in the denomination of *feldspar* only, which has been now too generally used not to be respected, and about which there can be no ambiguity, because it is no longer employed on any other occasion.

of the language of the science all the changes which should not produce too many inconveniences.

But in order the better to show the advantages of those which I have fixed upon, it will not be useless to explain, before any thing else, the principles to which, in my opinion, the formation of names independent of analysis ought to be subjected.

For a long time, persons have been in the habit of giving to mineral substances, names borrowed from those of the places where they were discovered. It seems to me that this is to invert the use of these names, which ought only to serve for designating individuals or particular bodies, as, when speaking of an *idiocrasis* the locality of which we wish to point out, we say that it is an idiocrasis from Vesuvius, or an idiocrasis from Siberia. If we substitute instead of *idiocrasis* the term *vesuvian*, which is adopted in Germany, the former expression will have the air of a pleonasm, and the second will appear contradictory.

Others derive the new denominations from the colour under which the substance is presented to the first observers. This is to transfer to the species the name of the variety. For example, *yanolite* (violet stone) is the substance which we call *axinite*. But there are crystals of this substance which are green, and in this case the name of green *yanolite* merely expresses an imaginary substance.

We ought also to avoid confounding the name of one mineral with that of another, with a different inflection; as when persons called *hyacinthine* the substance which we call *idiocrasis*, without doubt in order to recall the appearances ascribed to it in common with the hyacinth (zircon in the present system), with which it had been at first united. The truth is, that it differs from it very sensibly, either by its component principles or its structure, or even by the angles of its crystals; and it is sufficient to regard it for a moment in order to decide with Romé de l'Isle, that they ought to be separated from each other. So far therefore from indicating by the resemblance of the names a pretended connexion, which to good eyes does not exist, we ought rather to mark between the two substances a distinct line of separation, by a new name which could have nothing in common with the first, and which would make us forget, if possible, an error which mineralogists ought never to have committed.

With respect to insignificant names to which several naturalists give the preference, there is nothing to prevent their adoption. Of this number are the names derived from fable,

 such

such as titanium, uranium, &c. The ideas which they present are so far from carrying us to the objects which they serve to designate, that they can neither occasion mistake nor confusion; so that they are in the same situation as if they were purely imaginary. Sometimes also persons ascribe to a natural production the name of the person who discovered it; but it would be rather too severe to condemn entirely this way of paying a kind of homage for a favour conferred on science.

It appears to me that there is more advantage in employing significant words, which recall some characteristic property of the mineral to be denominated, or some circumstance relative to its history. But because this mineral is frequently distinguished from others only by its general characters, we ought not to require that the name, which can bear upon a single character only, should make the object it designates apparent without ambiguity. Moreover, if we consider that the characters of minerals are susceptible of variation, we must admit that the nomenclator may here allow himself considerable latitude, and it is sufficient that each word should rest on some idea connected with a knowledge of the object. Without this latitude, it would be almost impossible to make significant, *i. e.* reasonable, names. In a subject so fraught with difficulty, every thing is admissible, except what is inexcusable.

Now it must be confessed that the French language is not well adapted to furnish significant words without the help of periphrases, which exceed the narrow bounds within which true names ought to be confined. Let this language display in the descriptions of objects the clearness and precision which characterize it; but let the specific names be furnished by the Greek, which has the eminent advantage of moulding several words into one, so as to paint in miniature the object which it serves to denominate. In this way a multitude of words have been formed which are used in the arts and sciences. Every day these words are multiplied; the instrument which transmits ideas to a distance in the twinkling of an eye, is the *telegraph*; the art of writing written words with rapidity is called *stenography*, &c. Wherefore then should we banish the Greek language from the country of the sciences, where it has been so long naturalized, and where every new expression introduced by necessity, finds itself as it were in the same family with a thousand others which preceded it?

It is from the same source that I have derived the names which

which I have added to the nomenclature of mineralogy. Different motives suggested their formation; and there were two circumstances in particular, in which it was indispensable to compose new names: viz. when a species hitherto unknown was the subject of description, and when several different species had been confounded. In the latter case I generally left to one of the species the name which they had borne in common, and I designated the rest by particular denominations.

I was almost confined to these changes from absolute necessity, in the extract which formerly appeared of this treatise; and besides, I had allowed all the names already printed, to remain, however improper they might be. But, since, it has been observed to me that it would be proper to reform in the same manner several names which I had omitted, such as *leucite* and *leucolite*, one of which signifies a *white body*, and the other a *white stone*, *smaragdite*, which is nearly synonymous with *emerald*, *oisanite*, *andreolite*, *thallite*, and some others borrowed from the localities or from the colours. These names were found vicious in a twofold degree, either from their impropriety when considered in an isolated point of view, or from the monotony of their terminations when they happen to resemble each other closely. Besides all this, they were very few in number, and are only found in very modern works. In a word, it was thought that the interests of science, which had determined the first changes, would also suggest those which were proposed to me. I hesitated no longer from the moment I found myself supported by men of science whose reasons seemed to me to be decisive, and whose authorities alone have the weight of reasons; and I am the more anxious thus publicly to declare the motives which actuated me, because it would give me pain to be accused of allowing myself to be led away by neologism. I certainly think there is a great difference in every respect between making new names and advancing new theories. The one is the result of a mere technical labour, which interferes with the dictionary of science only; the other presupposes views which tend to aggrandize the edifice. A truth newly made known is instantly adopted, because it insinuates itself into the mind by means of persuasion. But the novelty alone of words which reach the ear for the first time, throws a shade of disapprobation over them: he who proposes them seems to assume an authoritative air; they are rejected without reflection and without examination, or they are censured perhaps, while at the same moment all are

agreed

agreed as to the utility of a change. But naturalists, who after long consideration undertake a task so painful, so fastidious, and so little calculated to indemnify them for their trouble, ought to have nothing but science in view, desiring no advantage but that of being useful, and dreading the reproach alone of not having done all that a true regard for science demands of them.

To conclude:—Those who would still cherish a predilection for the suppressed denominations, will find them in the same line with those which I have substituted, and may still continue to use them. But I hope that beginners, on comparing both nomenclatures, will store their memory, according to my plan, with names so constructed as to enlighten their minds. I have taken care to add to these names their etymologies, and I have done the same with all the rest, whether new or old, whenever there was a possibility of ascertaining their origin.

OF THE NOMENCLATURE OF CRYSTALS.

If the language of mineralogy has been so long defective, from the bad choice of specific names, the almost total deficiency of names with respect to the varieties of crystallization has left a void, which was no less an inconvenience. There was no exception, except with respect to a small number of these varieties, the forms of which were so simple that they would suggest as if of themselves the epithets of *cubical*, *octahedral*, *dodecahedral*, &c., which ought to be added to the names of the species. The more compound forms were indicated by definitions, the length of which was in some measure proportional to the number of the facets; or, if it was wanted to abridge these definitions, by borrowing them from a resemblance between the crystal and some familiar object *, this was done with so little rationality, that it would have been desirable for the honour of the comparison if such names were less known.

Convinced of the necessity of introducing the utmost precision into this part of mineralogical language, so much neglected hitherto, I have attempted to designate the various crystalline forms by simple and significant names, taken from the characters of these forms, or from the properties which result from their structure, and from the laws of decrement on which they depend. I shall here present my readers with the series of these names, under the form of a methodical system. I hope that those who peruse it with attention will find an assistant for engraving

* The following are examples of this kind: *nail-headed calcareous spar*, *dog-toothed calcareous spar*, &c.

these

these names on their memory, by connecting them with considerations which are easily classified in the mind. They will perceive that, by a kind of œconomy of language, extremely useful in such cases, the same name is frequently applicable to varieties taken in different species. It is true that on one hand the word which serves to designate such a variety might also serve another variety of the same species. For example: I denominate *binary*, a form which depends on a decrement by two ranges. Now supposing this decrement to take place on the edges, it is possible that another variety of the same substance may be owing to a decrement which takes place by two ranges on the angles. But in this case the system will present for the latter another name borrowed from a different consideration. The inconvenience just mentioned is common to all nomenclatures, and seems unavoidable. Thus, in the language of botany, one variety will bear the name of *crassifolia*, or of *rotundifolia*; while another variety of the same species shares with the first the character which has served to distinguish it. The essential requisite is, that the method should be copious enough to furnish at least to all the known wants of science. I even expect that, by means of the labour I have performed, a great part of the forms which shall be discovered in future will be found to have been named beforehand; and as to those which require new names, we shall have at least a system from which to designate them. In all descriptions of researches, it becomes easier to go forward when the route is traced.

Principles of the nomenclature.—The primitive form of any given substance is always designated by the word *primitive* added to the name of the species. Examples:—*primitive zircon, primitive carbonated lime, primitive sulphurated lime*, &c.

We may consider secondary forms:—

1. With respect to the modifications of the primitive form, when the faces of the latter are combined with those which result from the laws of decrement.
2. By themselves, and as purely geometrical forms.
3. With respect to certain facets or certain ridges remarkable by their assortment or their positions.
4. With respect to the laws of decrements on which they depend.
5. With respect to the geometrical properties which they present.
6. Finally, with respect to certain particular accidents.

1. *Secondary*

1. *Secondary forms considered with respect to the modifications which they present of the primitive form.*

The crystal is called,

a. *Pyramidated* (*pyramidé*), when the primitive form being a prism, has on each of its bases a pyramid which has as many faces as the prism has panes. Example: Pyramidated phosphated lime.

b. *Prismated* (*prismé*), when the primitive form being composed of two pyramids joined at their bases, these pyramids are separated by a prism. Ex. Prismated zircon, prismated quartz.

Semi-prismated, when there is only the half of the number of ridges situated around the common base, which are intercepted by panes. Ex. Semi-prismated sulphated lead.

c. *Based* (*basé*), when, the primitive form being a rhomboid, or an assemblage of two pyramids, the summits are intercepted by facets perpendicular to the axis, and performing the function of bases. Ex. Based carbonated lime, based sulphur.

d. *Pointed* (*epointé*), when all the solid angles of the primitive form are intercepted by solitary facets. Ex. Pointed mesotype.

We shall also use the terms *bi-pointed* (*bisépointé*), tripointed (*triépointé*), quadripointed (*quadriépointé*), according as each solid angle may be intercepted by two, three, or four facets. Ex. Tripointed analcime, quadripointed sulphurated iron.

e. *Marginated* (*emarginé*), when all the ridges of the primitive form are each of them intercepted by a facet. Ex. Marginated garnet.

We shall also use the term *bi-marginated, trimarginated,* as each ridge is intercepted by two or three facets. Ex. Trimarginated garnet.

f. *Peri-hexahedral, peri-octahedral, peri-decahedral, peri-dodecahedral,* when the primitive form being a prism with four panes, is changed by the effect of decrements into a hexahedral, octahedral, decahedral, or dodecahedral prism. We also denominate *peri-dodecahedron* a crystal the nucleus of which being a regular hexahedral prism, has its six longitudinal ridges intercepted by as many facets. Ex. Peri-hexahedral sulphated copper, peri-dodecahedral emerald.

g. *Recurved* (*raccourci*), when the primitive form being a prism with rhombic bases, the longitudinal ridges contiguous

tiguous to the grand diagonal are intercepted by two facets, which make it appear diminished in the direction of its length. Ex. Recurved sulphated barytes.

h. *Retreated* (*rétréci*), when the primitive form being a prism with rhombic bases, the longitudinal ridges contiguous to the small diagonal are intercepted by two facets which make it appear diminished in the direction of its breadth. Ex. Retreated sulphated barytes.

2. *Secondary forms considered in themselves, and as being purely geometrical.*

The crystal is called,

a. *Cubical*, when it presents the form of the cube, which in this case is always secondary. Ex. Cubical fluated lime.

b. *Cuboidal*, when its form differs a little from the cube. Ex. Cuboidal carbonated lime.

c. *Tetrahedral*, when it presents the form of the regular tetrahedron, as a secondary form. Ex. Tetrahedral sulphurated zinc.

d. *Octahedral*, when it presents the form of this solid, as secondary. Ex. Octahedral muriated soda.

e. *Prismatic*, when it has the form of a straight or oblique prism, the panes of which are inclined 120° among each other. Ex. Prismatic carbonated lime, prismatic feldspar.

f. *Dodecahedral*, when its surface is composed of twelve triangular, quadrangular, or pentagonal faces, all equal and similar, or solely of two measurements of different angles. Ex. Dodecahedral quartz, dodecahedral zircon, dodecahedral sulphurated iron.

If the dodecahedron had not all its faces of the same number of sides, it would be sufficient to bringthemto this aspect in imagination, by varying its dimensions.

g. *Icosahedral*, when its surface is composed of twenty triangles, of which twelve are isosceles and eight equilateral. Ex. Icosahedral sulphurated iron.

h. *Trapezoidal*, when its surface is composed of twenty-four equal and similar trapezoids. Ex. Trapezoidal garnet.

i. *Triacontahedral*, when its surface is composed of thirty rhombuses. Ex. Triacontahedral sulphurated iron.

k. *Enneacontahedral*, when its surface is composed of 90 faces. Ex. Enneacontahedral idiocrasis.

l. *Birhomboidal*, when its surface is composed of twelve faces, which being taken by sixes, and lengthened in imagination until they intersect, would form two different rhomboids. Ex. Birhomboidal carbonated lime.

We

We say *trirhomboidal* in the same manner. Ex. Trirhomboidal carbonated lime.

m. *Biform*, *triform*, when it contains a combination of two or three remarkable forms; such as the cube, the rhomboid, the octahedron, the regular hexahedral prism, &c. Ex. Triform sulphated alumine.

n. *Cubo-octahedral*, *cubo-dodecahedral*, *cubo-tetrahedral*, &c., when it contains a combination of the two forms indicated by these expressions. Ex. Cubo-octahedral fluated lime, cubo-dodecahedral sulphurated iron, cubo-tetrahedral gray copper.

o. *Trapezian*, when its lateral surface is composed of trapezia situated on two rows between two bases. Ex. Trapezian sulphated barytes.

p. *Ditetrahedral*, i. e. twice tetrahedral, when its form is that of a tetrahedral prism with dihedral summits. Ex. Ditetrahedral grammatite.

q. *Dihexahedral*, when it forms a hexahedral prism with trihedral summits. Ex. Dihexahedral feldspar.

We say in the same manner, *dioctahedral*, *didecahedral*, *didodecahedral*. Ex. Dioctahedral topaz, didecahedral feldspar, didodecahedral phosphated lime.

r. *Trihexahedral*, *tetrahexahedral*, *pentahexahedral*, *heptahexahedral*, when its surface is composed of three, four, five, seven rows of facets disposed in sixes the one above the other. Ex. Trihexahedral nitrated potash, pentahexahedral quartz, heptahexahedral nitrated potash.

We also say in the same manner, *tridodecahedral*. Ex. Tridodecahedral sulphurated antimoniated silver. —*Trioctahedral*. Ex. Trioctahedral sulphated lead.

s. *Bigeminated*, when it presents a combination of four forms, which, taken by twos, are of the same species. Ex. Bigeminated carbonated lime.

t. *Amphihexahedral*, i. e. *hexahedral* in two ways, when by taking the faces according to two different directions, we have two hexahedral contours. Ex. Amphihexahedral axinite.

u. *Sexdecimal*, when the faces which belong to the prism or to the middle part, and those which belong to the two summits, are the former six in number, and the latter ten in number, or *vice versâ*. Ex. Sexdecimal feldspar.

In the same manner we say *octodecimal*. Ex. Octodecimal feldspar. *Sexduodecimal*. Ex. Sexduodecimal carbonated lime. *Octoduodecimal*. Ex. Octoduodecimal

duodecimal sulphated copper. *Deciduodecimal.* Ex. Deciduodecimal feldspar.

x. *Peripolygonal,* when the prism has a great number of panes. Ex. Peripolygonal tourmaline.

y. *Supercomposite,* when the form is very much compounded. Ex. Supercomposite tourmaline.

z. *Antienneahedral,* i. e. having nine faces on two opposite sides, is a name peculiar to a variety of the tourmaline, in which the two summits are of nine faces, and the prism of twelve panes; whereas, generally, the prism is enneahedral.

aa. *Prosenneahedral,* i. e. having nine faces on two adjacent parts, is another variety of the tourmaline, in which the prism and one of the two summits have each nine faces.

bb. *Recurrent,* when, on taking the faces of the crystal by annular rows, from one extremity to the other, we have two numbers, which succeed several times, as 4, 8, 4, 8, 4. Ex. Recurrent oxidated tin.

cc. *Equidifferent,* when the numbers which designate the faces of the prism and those of the two summits, which in this case differ from each other, form the commencement of an arithmetical series, as 6, 4, 2. Ex. Equidifferent amphibolus.

dd. *Convergent,* when in the foregoing case the series is sensibly convergent, as 15, 9, 3. Ex. Convergent tourmaline.

ee. *Unequal* (*impair*), when the numbers which designate the panes of the prism and the faces of the two summits, considered as different from each other, are all three unequal, without being in other respects in progression. Ex. Unequal tourmaline.

ff. *Hyper-oxidated,* i. e. *acute to excess,* is a variety of carbonated lime, which contains the combination of two rhomboids; the one acute, which is the inverse; the other incomparably more acute.

gg. *Spheroidal,* is said of the diamond with 48 bombated faces.

hh. *Plano-convex,* is the diamond with some plane and some curvilinear faces.

3. *Secondary forms considered relatively to certain facets, or certain ridges, remarkable for their arrangement or position.*

The crystal is called,

a. *Alternate,* when it has on its two parts, the one superior

rior and the other inferior, faces which alternate among each other, but which correspond on both sides. Ex. Alternate quartz.

Bisalternate, when in the foregoing case the alternation takes place, not only among the faces of one and the same part, but also among those of the two parts. Ex. Bisalternate carbonated lime, bisalternate quartz.

Bibisalternate, when there are on both sides two orders of bisalternate facets. Ex. Bibisalternate sulphurated mercury.

b. *Annulary*, when a hexahedral prism has six marginal facets ranged in form of a ring around each base. Ex. Annulary emerald.

We say the same of an octahedral prism with eight marginal facets around bases. Ex. Annular oxidated tin.

c. *Monostic*, when a prism of any given number of panes has, in the contour of each base, a row of facets in number different from that of the panes, and which may be all marginal, or some marginal and others angular. Ex. Monostic topaz.

Distic, when in the same case there are two rows of facets around each base. Ex. Distic topaz.

Subdistic, when among the facets arranged on one and the same row around each base, two are surmounted each by a new facet, which is as it were the rudiment of a second row. Ex. Subdistic peridot.

d. *Plagihedral*, when the crystal has facets situated in a slanting direction. Ex. Plagihedral quartz, plagihedral zircon.

e. *Dissimilar*, when two rows of facets, situated the one above the other, towards each summit, have a defect in symmetry. Ex. Dissimilar topaz.

f. *Squared* (*encadré*), when it has facets which form kinds of squares around faces of a simpler form already existing in the same species. Ex. Squared fluated lime.

g. *Slightly prominent* (*prominule*), when it has ridges which form a very slight eminence. Ex. Slightly prominent sulphated lime.

h. *Zonary*, when it has around its middle part a row of facets, which form a kind of zone. Ex. Zonary carbonated lime.

i. *Apophanous*, i.e. *manifest*, when certain facets or certain ridges present some indication useful for ascertaining the position of the nucleus, which would otherwise be difficult to find out, or even to determine, either in point

point of direction or the measurement of the decrements. Ex. Apophanous feldspar, apophanous sulphurated antimoniated silver, apophanous gray copper.

l. *Blunted* (*emoussé*), when it has facets which intercept, and render as if blunted, some parts which would otherwise be sharper than the rest. Ex. Blunted axinite, blunted carbonated lime.

m. *Contracted*, is a dodecahedral variety of carbonated lime, in which the bases of the extreme pentagons undergo a kind of contraction, in consequence of the inclination of the lateral faces.

n. *Dilated*, is said of another dodecahedral variety of carbonated lime, in which the bases of the extreme pentagons undergo a kind of dilatation, in consequence of the inclination of the lateral faces.

o. *Acuteangled*, is a variety of carbonated lime in a hexahedral prism, the solid angles of which are intercepted by very sharp triangular facets.

p. *Defective*, is a variety of borated magnesia, in which four solid angles of the primitive cube are intercepted by facets, while the opposite angles remaining untouched, are subject to a kind of defect.

q. *Superabundant*, is another variety of borated magnesia, in which the solid angles which were untouched in the defective variety, are intercepted each by four facets, in such a way as to make a superabundance where there was a defect.

4. *Secondary form sconsidered relatively to the laws of decrement on which they depend.*

The crystal is called,

a. *Unitary*, when it undergoes only a single decrement by one row. Ex. Unitary telesia. If there are two, three, four decrements by one row, we say bisunitary, triunitary, quadriunitary. Ex. Triunitary peridot, bisunitary carbonated lime.

b. *Binary, bibinary, tribinary*, &c. in the case of one, two, and three decrements by two rows. Ex. Binary oligistous iron, bibinary feldspar.

c. *Ternary, biternary*, &c. in the case of one, two decrements, &c. by three rows.

d. *Unibinary*, if there are two decrements, the one by one row, the other by two; *uniternary*, if there is one by one row, and the other by three; *binoternary*, if there is one by two, and the other by three, &c. Ex. Uniternary carbonated lime, binoternary carbonated lime.

The nomenclature in all the foregoing expressions, as well as in those which follow, makes an abstraction of the faces parallel to those of the nucleus, which exist most frequently in the secondary crystal. Among the forms in which the nucleus is entirely disguised, some have names borrowed from different considerations; and those which remain are so few in number, that I thought it unnecessary to complicate the language by employing a particular designation for them.

In order to avoid confounding the words which express the decrements with those which indicate the number of the faces, we may remark, that the former have their termination in *hedral*, as dodecahedral, or in *al*, as octodecagonal, whereas the others end in *ary*.

e. *Equivalent*, when the part visible (*exposant*) which indicates a decrement is equal to the sum of those which indicate the others. Ex. Equivalent sulphated iron.

f. *Subtractive*, when the part visible relative to a decrement is less by unity than the sum of those which indicate the others. Ex. Subtractive pyroxene.

g. *Additive*, when the part visible relative to a decrement exceeds by unity the sum of those which indicate the others. Ex. Additive sulphated copper.

h. *Progressive*, when the parts visible form a commencement of arithmetical progression; as 1, 2, 3. Ex. Progressive tourmaline.

i. *Disjointed*, when the decrements form an abrupt leap, as from 1 to 4 or to 6. Ex. Disjointed sulphurated antimoniated silver.

k. *Partial*, when there is some part which remains without decrements, while the other parts similarly situated undergo them. Ex. Partial sulphurated cobalt.

l. *Subdouble*, when the part visible relative to a decrement is the half of the sum of the other parts visible. Ex. Subdouble topaz.

We say *subtriple*, *subquadruple*, &c. in the same way. Ex. Subtriple sulphated copper.

The three parts visible (*exposans*) which compose the indication of an intermediary decrement, count as one only, which is equal to their sum.

m. *Doubling*, *tripling*, *quadrupling*, when one of the visible parts is repeated twice, thrice, or four times in one series which would otherwise be regular. Ex. Doubling peridot, quadrupling peridot.

n. *Identical*, when the parts visible of the simple decrements, to the number of two, are equal to the terms of the fraction

fraction relative to a third decrement which is mixed. Ex. Identical gray copper.

o. *Isonomous*, i. e. *equality of laws*, when the parts visible which indicate the decrements on the edges being equal, those which express the decrements on the angles are equal also. Ex. Isonomous sulphated copper.

p. *Mixed*, when the form results from a single mixed decrement. Ex. Mixed telesia.

q. *Pantogenous*, i. e. *deriving its origin from all the parts*, when each ridge and each solid angle undergoes a decrement. Ex. Pantogenous sulphated barytes.

r. *Biferous*, i. e. *which carries twice*, when every ridge and every solid angle undergoes two decrements. Ex. Biferous gray copper.

s. *Surrounded*, (*entouré*) when the decrements take place on all the ridges and on all the solid angles around the base of a prismatic nucleus. Ex. Surrounded sulphated barytes.

t. *Opposite*, when a decrement is made by one row, and another is intermediary. Ex. Opposite oxidated tin.

u. *Synoptic*, when the laws of decrement present as it were the picture of those which take place with respect to the whole of the other crystals, or at least with respect to the greatest part. Ex. Synoptic feldspar.

x. *Retrograde*, is a variety of carbonated lime, the expression of which contains two mixed decrements, which are such that the faces resulting from them seem to retrograde, by throwing themselves backward, on the side of the axis opposite to that which looks towards the face on which they originate.

y. *Ascending*, when all the laws of decrement have an ascending course, setting out from the angles or lower edges of a rhomboidal nucleus. Ex. Ascending carbonated lime.

5. *Secondary forms considered relatively to the geometrical properties which they present.*

The crystal is called,

a. *Isogonous*, i. e. *equality of angles*, when the faces which are on parts differently situated form equal angles between each other. Ex. Isogonous cymophane.

b. *Anamorphic*, i. e. *form turned upside down*, when we cannot give it the position most natural to it, without that of the nucleus being as it were turned upside down. Ex. Anamorphic stilbite.

c. *Rhombiferous*, when certain facets are true rhombuses,

although,

although, from the manner in which they are cut by the adjoining faces, they do not appear at the first glance to be of a symmetrical figure. Ex. Rhombiferous quartz.

d. Equiaxis, when it has the form of a rhomboid the axis of which equals that of the primitive rhomboid. Ex. Equiaxis carbonated lime.

e. Inverse, when it has the form of a rhomboid the salient angles of which are equal to the plane angles of the primitive rhomboid, and *vice versâ.* Ex. Inverse carbonated lime.

f. Metastatic, i. e. *transferred,* when it has plane angles and solid angles equal to those of the nucleus which are thus transferred to the secondary form. Ex. Metastatic carbonated lime.

g. Contrasting, when it has the form of a very acute rhomboid, in which an inversion of angles similar to that which takes place in the inverse (letter *e*) presents a kind of contrast, in so far as it resembles in another part a very obtuse rhomboid. Ex. Contrasting carbonated lime.

h. Persisting, is a variety of carbonated lime in which certain faces are cut by the adjoining faces, so that they preserve the same measurements of angles which they would have had without that, except that these angles have other respective positions. Ex. Persisting carbonated lime.

i. Analogic, when its form presents several remarkable analogies. Ex. Analogic carbonated lime.

k. Paradoxal, when its structure presents singular and unexpected results. Ex. Paradoxal carbonated lime.

l. Complex, when its structure is complicated by laws not very common, as when it is produced by decrements some mixed and others intermediary. Ex. Complex carbonated lime.

6. *Secondary forms considered relatively to certain particular accidents.*

The crystal is called,

a. Transposed, when it is composed of two halves of an octahedron, or of two portions of another crystal, one of which seems to have turned upon the other in a quantity equal to a sixth of its circumference. Ex. Transposed spinel, transposed sulphurated zinc.

b. Hemi-trope, i. e. *one-half reversed,* when it is composed of two halves of one and the same crystal, one of which seems to be reversed. Ex. Hemi-trope feldspar.

c. Rectangular,

c. *Rectangular*, a particular name given to the staurotide composed of two prisms which cross at right angles.

d. *Obliqueangled*, a particular name given to the staurotide composed of two prisms which cross at an angle of 60°.

e. *Sexradiated*, a name given to the staurotide composed of three prisms which cross so as to represent the six radii of a regular hexagon.

f. *Cruciform*, a name given to the harmotome composed of two crystals which form a kind of cross.

g. *Triglyphous*, when the striæ considered on three faces united around one and the same solid angle, are in three directions perpendicular to each other. Ex. Triglyphous sulphurated iron.

h. *Geniculated*, when it is composed of two prisms which unite by one extremity forming a kind of knee. Ex. Geniculated oxidated titanium.

In the descriptions of the species we shall meet with a small number of denominations which we have here omitted. But their signification will be immediately obvious, or will resolve itself into that of some of the foregoing denominations.

[To be continued.]

XLVI. *On a Method of examining the Divisions of astronomical Instruments. By the Rev.* William Lax, *A.M., F.R.S.,* Lowndes's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* Maskelyne, *F.R.S. Astronomer Royal**.

St. Ibbs, August 27, 1808.

Dear Sir, I am persuaded that you must feel, in common with myself, how unpleasant it is to make use of an instrument in astronomical observations requiring extreme accuracy, whose exactness you have no adequate means of ascertaining, but are obliged to depend for it in a great measure upon the abilities and integrity of the artist. It is in vain that we observe with so much nicety, and read-off with so much precision, if we are still uncertain whether there may not be an error in the instrument itself of much greater magnitude than those which we are endeavouring to prevent; and that our best instruments must be liable to such errors, no person can possibly doubt, who has paid due attention to the sources from whence they may arise. I have estimated, as accurately as I could, the amount to which they may accumulate in Bird's method of dividing by continual bisections, and have satisfied myself that they are much more considerable than is generally

* From the Philosophical Transactions for 1809, Part II.

appre-

apprehended: but as I cannot obtain such precise information as I could wish, respecting the exactness with which a bisection can be performed, or a length taken from the scale of equal parts and laid upon the instrument, I will not trouble you with the deduction which I have made. It is understood, indeed, that Bird's method is now generally laid aside, and that each artist employs one, which he considers in many respects as peculiar to himself; but I presumed that there would still be such a connexion betwixt Bird's method and those which have been substituted in its stead, as to render them in some degree liable to the same errors to which it was subject; and the reports which I have uniformly received from persons, who have had an opportunity of examining some of the modern instruments, have fully convinced me that my opinion was just. But whatever may be the nature of the methods which are now in use, or whatever their advantages over Bird's, I never could persuade myself that it would be safe to trust to an instrument, without a previous examination. To discover the means of accomplishing this object, is what I have for some time been anxious to effect; and though I fear my endeavours have not been very successful, I will nevertheless take the liberty of presenting you with the result.

You are aware, I believe, that I use a circular instrument for observing both in altitude and azimuth, which was made for me by Mr. Carey in the Strand; that the radius of both the altitude and the azimuth circle is one foot, and that each is divided into parts containing ten minutes. The construction of this instrument does not differ materially from that of other similar instruments, with which you are well acquainted, and I shall not therefore waste your time by giving you a particular description of it. For the purpose of examining the divisions upon the two circles, I procured an apparatus to be prepared by Mr. Carey, which will be very easily explained. To the face of the rim which surrounds the azimuth circle, and with its left end close to the stand which supports the micrometer on the east side, an arc of brass, concentric with the circle itself, and a little more than 90° in length, an inch in breadth, and one eighth of an inch in thickness, is firmly fixed by screws, so as to have the plane parallel to the plane of the circle; and a small portion of its lower surface resting upon the extreme part of the rim. The screws pass through a brass arc, which is fastened to this at right angles, and lies with its broad side against the face of the rim. Upon the first-mentioned arc, a strong upright piece of brass, about six

six inches in length, is made to slide, the lower part of it embracing the arc as a groove, and having a clamping screw underneath, for the purpose of fixing it firmly to the arc at any point required. To the top of the upright piece of brass is attached a microscope, with a moveable wire in its focus, pointing down to the division upon the circle, not directly, however, but with an inclination to the left of about 30°. This inclination is given to it, in order to make it point to the same division upon the circle, which is immediately under the micrometer itself, when it has been moved up as near to the micrometer as it is capable of approaching. The microscope has attached to it a small graduated circle of brass, and an index, by which the seconds, and parts of a second, moved over by the wire are determined.

To the vertical circle there is likewise an arc applied, of the same length and breadth as the former, but somewhat thicker, and of a radius exceeding that of the circle by about two inches. This greater thickness is given to it, on account of its being supported in a manner which renders additional strength necessary. It is fixed with its broad convex side downwards upon two brass pillars, screwed fast to the plane of the azimuth circle, and standing in a line parallel to the plane of the vertical circle at the distance of about four inches from it, and on the right side of the pillars which support the micrometers belonging to this circle. The pillar, to which the left end of the arc is fastened, is placed close to the lower micrometer of the vertical circle, and the other contiguous to the elevated rim, in which the divisions of the azimuth circle are cut. The right end of the arc reaches beyond this pillar about ten inches. The pillars are of such a height, and so proportioned to each other, that whilst the left end of the arc, which lies horizontally, is raised to within about two inches of the height at which the lowest point of the vertical circle is placed, the whole arc runs parallel to the circle through an extent of something more than 90°. Upon the arc a microscope, with a moveable wire in the focus, is made to slide as in the former case, and to point to the divisions upon the vertical circle, not directly, but with an inclination of about 30° to the left, in order that the same division (which is the lowest upon the circle) may be seen through it and through the lower micrometer at the same time.

I will now proceed to show you in what manner the examination of the divisions upon either circle may be

 performed.

performed. The process is precisely the same in both cases, and will of course be described in the same words.

The first point to be examined is that of 180°, which must be done in the usual way, by bringing the points of 0 and 180° to the moveable wires of the opposite micrometers, and then turning the circle half-way round, and bisecting the points again with the moveable wires; and lastly, taking half the difference betwixt the distances of the wires in the two positions of the circle for the error at the point of 180°. Having now bisected the point of *zero* with the moveable wire of the micrometer, which is intended to be used in the rest of the process (for we shall have no further occasion for both), we must slide the microscope along the arc, till by moving the wire a little we can bisect the point of 90°, and then the micrometer must be firmly clamped to the arc. The circle must then be turned till the point of 180° is brought to the microscope, and that of 90° to the micrometer, so that we may be able to bisect each by a slight motion of their respective wires. This being done, we must observe, from the positions of the wires, how much the interval betwixt them has increased or decreased in the measurement of the new arc; and this increase or decrease must be noted down with a + or − accordingly. In the same manner we must proceed through the remaining two arcs of 90°, observing and noting down the difference betwixt each and the original arc.

The point of *zero* must now be brought again to the micrometer, and bisected by the moveable wire, and the microscope be made to slide back along the arc, till by moving the wire a little we can bisect the point of 60°; and when this is done, the microscope must be clamped. We must then measure the arc of 60° against every succeeding arc of 60° in the circle, precisely in the same way that we measured the first arc of 90° against the other three. The arc of 45° is next to be measured against every succeeding arc of 45°, and this will complete all that is necessary to be done in the early part of the morning before the heat of the sun can have affected the temperature of the instrument. The rest may be performed at our leisure.

You will immediately perceive the object of this kind of measurement. It enables us to determine, with any degree of accuracy that may be required, the proportion which the first and every succeeding arc of the circle, contained betwixt the micrometer and the microscope, bears to the whole circle, and of course the absolute length of the

the arcs themselves. Let a denote the real length of the first of these, and $\pm a'$, $\pm a''$, $\pm a'''$, &c., the difference betwixt the first and second, the first and third, &c. respectively; let A represent any other arc whose length is known, and which is a multiple of a, as marked upon the instrument, and let this multiple be expressed by n. Then will $a + (a + a') + (a + a'') + (a + a''') + \&c. \ldots (a + a'''^{\ldots\overline{n-1}}) = A$, and $a = \frac{A - a' - a'' - \ldots a'''^{\ldots\overline{n-1}}}{n}$. Hence it is evident, that if there is no error committed in the measurement of any of these arcs, we shall have the value of a, and consequently of $a + a'$, $a + a''$, $a + a'''$, &c., and of any arc, comprehending any number of these, accurately determined. But if there be an error of e in the measurement of the first, of e', e'', e''', &c., in the measurement of the second, third, &c., respectively, then we shall have the following equation for determining a, viz. $a + (a + a' + e + e') + (a + a'' + e + e'') + \&c. \ldots (a + a'''^{\ldots\overline{n-1}} + e + e'''^{\ldots\overline{n-1}}) = A$, and consequently a will appear to be equal to $\frac{A - a' - a'' - \ldots a'''^{\ldots\overline{n-1}} - \overline{n-1}\, e - e' - e'' - \ldots e'''^{\ldots\overline{n-1}}}{n}$, which differs from its true value by $\frac{\overline{n-1}\, e + e' + e'' + \ldots e'''^{\ldots\overline{n-1}}}{n}$. Hence it follows, that the value of the p^{th} arc (p being greater than unity), as deduced by this process, will differ from its true value by $\frac{\overline{n-1}. e + e' + e'' + \ldots e'''^{\ldots\overline{p-1}} + e'''^{\ldots p} + \ldots e'''^{\ldots\overline{n-1}}}{n} - e - e'''^{\ldots\overline{p-1}}$, and that if we add any number p of these arcs together, in order to determine the value of the arc which is equal to their sum, we shall have an error in this value (and the expression holds when p is unity, or the first arc only is taken) equal to $p\,\frac{\overline{n-1}. e + e' + e'' + \ldots e'''^{\ldots\overline{p-1}} + e'''^{\ldots p} + \ldots e'''^{\ldots\overline{n-1}}}{n} - \overline{p-1}. e - e' - e'' - \ldots e'''^{\ldots\overline{p-1}} = \frac{\overline{n-p}. e - e' - e'' - \ldots e'''^{\ldots\overline{p-1}} + p. e'''^{\ldots p} + e'''^{\ldots\overline{p+1}} + \ldots e'''^{\ldots\overline{n-1}}}{n}$. Now, if we suppose e to be the greatest error to which we are liable in the measurement of any arc, and each of the succeeding errors to be equal to it, and likewise that e', e'',

$\ldots e'''$

$\therefore e'''\cdots\overline{p-1}$ are all negative, then it will appear that $\frac{n-p}{n}\times$ $2pe$ will be the greatest error that can be committed in determining the value of any arc by adding together the values of the (p) smaller arcs of which it is compounded. For instance, if the interval betwixt the micrometer and the microscope comprehends an arc of 60°, as marked upon the instrument, and this arc is measured against every succeeding arc of 60° in the whole circle, we shall have the greatest error that can be committed in deducing the arc of 120° from the addition of the two first arcs of 60°, equal to $\frac{6-2}{6}\times 2\times 2e = 2{\cdot}66\,e$. After these remarks, we may proceed to consider how the remaining divisions upon the circle may be examined with the least probable error, and to ascertain the amount of the greatest to which the process can in any case be liable.

Let the arc of 30° be now measured against every succeeding arc of 30° in the first, third, fourth, and sixth arcs of 60°, and let the length of each be determined from a separate comparison with the arc of 60°, in which it is comprehended, and not from a general comparison with all the four. The arc of 15° must then be measured against every succeeding arc of 15° in the first, third, fourth, sixth, seventh, ninth, tenth, and twelfth arcs of 30°, and the value of each deduced from a comparison with the arc of 30°, in which it is contained. When this is done, we shall have determined the length of every succeeding arc of 15°, of the first arcs of 30, 45, 60, 75 (= 60 + 15), 90, 105 (= 90 + 15), 120 (= 60 + 60), 135 (= 90 + 45), 150 (= 120 + 30), 165 (= 150 + 15), and 180° in each semicircle.

We must next measure the arc of 5° against every succeeding arc of 5° in the whole circle, and deduce the values of the first, and of the sum of the first and second, in each succeeding arc of 15°, from a comparison with the arc of 15° in which they are contained. We must then proceed to determine the values of the first arc of 3° in each 15°, and of its multiples the arcs of 6, 9, and 12°. We must also put down the value of the last arc of 3° in each arc of 15°, and then deduce the values of the first and last arcs of 1° in each arc of 15°, from a comparison with the arc of 3° in which they are respectively contained.

We shall now have measured in each arc of 15° the first arcs of 1, 3, 5, 6, 9, 10, 12°, and by taking the last arc of one degree, which has likewise been determined, from the arc

arc of 15°, we shall obtain the first arc of 14°. The first 7° of this arc being measured against the second, we ascertain the value of the first 7°; and then, by measuring the first 4° of the remaining arc of 8° against the second, we shall get the value of the first 4°, which added to the arc of 7°, before determined, will give us the length of the first arc of 11°. The first 2° of the remaining arc of 4° must then be measured against the second, and we shall get the value of the first 2°, and by adding this arc to the arc of 11°, we shall obtain the value of the arc of 13°. By taking away the first arc of 1° from the arc of 15°, we get the remaining arc of 14°; and then having determined the length of the first 7° of this arc, by measuring them against the second, we must add it to the arc of 1°, and we shall obtain the arc of 8°. The length of the first 4° of this arc will then be easily known, by measuring them against the second, as will afterwards that of the first 2° in the arc of 4° itself, by measuring them against the second in the same arc.

We have still to ascertain the lengths of all the first arcs of 10, 20, 30, 40, and 50 minutes contained in each degree, for I shall only consider the case in which the circle is divided into parts of 10 minutes. Now the length of the first arc of 30′ will be obtained by measuring it against the second, and the lengths of the first and second arcs of 20′ (whose sum will give the arc of 40′) by measuring the first against each of the remaining arcs. The length of the third arc of 20′ must likewise be put down, and then the first arc of 10′ being measured against the second of the arc of 20′, in which it is included, and also against the two arcs of 10′ contained in the last arc of 20′, its own value, and that of the last 10′ in the degree will be determined from a comparison with the arcs of 20′, in which they are respectively comprehended. The length of this last arc of 10′ being taken from that of the whole degree, will give us the length of the first 50′, and complete the operation.

In order to ascertain the greatest possible error to which we are liable in the examination, let ε denote in parts of a second the greatest that can be committed in bisecting any point upon the limb; then, since this error may occur at each end of the arc, it is evident that e in the expression deduced above $\left(\frac{n-p}{n} \times 2pe\right)$ will become 2ε, and the expression itself $\frac{n-p}{n} \times 4p\varepsilon$. Hence the possible error will be

be $\frac{2-1}{2}$ $4\varepsilon = 2\varepsilon$ at 180°; $\frac{2\varepsilon}{2} + \frac{2-1}{2} \times 4\varepsilon = 3\varepsilon$ at 90°; $\frac{2\varepsilon}{3} + \frac{3-1}{3} \times 4\varepsilon = 3{\cdot}33\varepsilon$ at 60°; $\frac{2}{3} \times 2\varepsilon + \frac{3-2}{3} \times 4 \times 2\varepsilon = 4\varepsilon$ at 120°. The greatest error must therefore lie betwixt 90 and 120°, and nearer to the extremity of the latter than of the former arc. At 105° it will be $5{\cdot}50\varepsilon$; at 111° it will be $5{\cdot}50\varepsilon - \frac{2}{5} . 1{,}5\varepsilon + \frac{5-2}{5} \times 4 \times 2\varepsilon = 9{,}70\varepsilon$; and at 111° 10′ it will be $9{,}70\varepsilon - \frac{1}{6} . 1{,}04\varepsilon$ (the excess of the error at 111° above that at 112°) $+ 3{\cdot}33\varepsilon = 12{\cdot}86\varepsilon$, which will be found to be the greatest error betwixt 105 and 120°, and of course the greatest in the first semi-circle. In the other semi-circle, the process being the same, the possible errors must necessarily be the same at the same distances from the first point, reckoning the contrary way upon the circle.

The magnitude of the quantity ε will of course vary upon circles of the same radius, according to the excellence of the glass employed, and the accuracy of the examiner's eye. It will seldom, however, exceed one second upon a circle whose radius is one foot; and in general it will not amount to so much. I find that I can read off, to a certainty, within less than three fourths of a second; and hence I conclude, that I could examine the divisions of my circle without being liable to a greater error than 9·63 seconds, and those of a circle of three feet radius without the risk of a greater error than 3·21 seconds.

To those people who are accustomed to entertain such exalted notions of the accuracy with which astronomical instruments can with a certainty be divided, this error, I dare say, will appear very considerable; but for my part, I am perfectly satisfied that it bears but a small proportion to the accumulated error which may take place, in spite of the utmost vigilance of the artist, in an instrument divided according to any method which has hitherto been made public. I need not, however, remark upon the very great improbability that the error of examination should ever attain, or approach, to its extreme limit, as this must be sufficiently obvious to any person who is in the least degree conversant with the doctrine of chances; but it may be proper to observe, that we have it in our power (and in this respect the examiner possesses a most important advantage over the divider of an instrument) to diminish its probable amount, as much as we please, by bringing the moveable wires of the micrometer and microscope several times

times to bisect their respective points in the measurement of every arc, and taking a mean of the different *readings-off* for the true position of the wire at the real bisection of the point. The wire may be moved in this manner eight or ten times at each point (if such a degree of caution should be thought necessary) and the mean taken in little more than a minute; so that the time of performing the work will not be so much increased as might perhaps have been apprehended; and when it is completed, we may reasonably presume that the distance of every point from zero (whilst the temperature of the circle continues uniform) will have been determined with sufficient exactness for every practical purpose.

Of the time necessary for the examination, a pretty correct idea may be formed by considering how many measurements are required, and allowing about a minute and a half for each; *i. e.* a quarter of a minute for bringing the extreme points of the arc to the micrometer and the microscope, and a minute and a quarter for making the several bisections. Now, in dividing the whole circle into arcs of 15° each, it will appear that forty-four measurements must be performed; and to examine every point in each arc of 15°, there will be 161 required, making in all 3908 measurements; and consequently the time necessary for completing the whole work will be 5862 minutes, or about 98 hours.

The time and labour required for this examination are, no doubt, very considerable; but it ought to be recollected, that it will render any great degree of precision, in dividing the instrument, totally unnecessary. Whoever indeed employs this method of examination, will be virtually the divider of his own instrument; and all that he will ask of the artist, is to make him a point about the end of every five or ten minutes, whose distance from zero he will determine for himself, and enter in his book to be referred to when wanted. We may likewise observe, that by this examination we shall not only be secured against the errors of division, but against those which arise from bad centering, and from the imperfect figure of the circle, and which in general are of too great a magnitude to be neglected.

It will, I dare say, have occurred to you, that whenever we are desirous that an observation should be particularly exact, we may guard it against the effects of unequal expansion or contraction in the metal, by means of the apparatus which I have described: for we have only to measure the arc which has been determined by the observation against

against the whole circle, or against the multiple of it, which approaches nearest to the circle, and from thence to deduce its value in the manner explained above, and we shall either have entirely excluded the error which we apprehended, or have rendered it too small to be of any importance. Suppose, for instance, that the arc determined by the observation was 48°; then by measuring it against the whole circumference increased by an arc of 24°, we shall obtain a result free from any greater error of unequal temperature, than one eighth of the increase or decrease of this arc of 24° beyond a due proportion to that of the circle itself.

This expedient gives us all the advantages of the French circle of repetition, without the inconvenience arising from being obliged to turn the instrument, and move the telescope, so many times in the course of the observation. Nay, I am persuaded that the result may be made more accurate in this way than by the French method, because not only can the object be more frequently observed, but the contacts or bisections, it may be presumed, will be more exact when the observer is not disturbed by the hurry attendant upon the use of the repeating circle; and with respect to any error in the instrument, from whatever cause it may arise, it will be as effectually excluded by the process which I recommend, as by moving the telescope round the circle. Besides, this method is applicable either to the azimuth or altitude circle, or indeed to any circle which turns upon its own axis; whereas the French method can never be applied to the azimuth circle, nor to any other circle which does not turn both upon its own axis and upon one which is perpendicular to it.

After all, however, it is possible that the process which I have been explaining to you may be no new discovery, and that you may be already acquainted with it. If this should be the case, you will be kind enough to inform me. At any rate, indeed, I should esteem myself greatly obliged, if you would favour me with your sentiments upon the subject, as soon as you can do it with perfect convenience to yourself.

I am, dear sir, yours, &c.

William Lax.

XLVII. On

XLVII. *On Azimuthal Refraction.*

To Mr. Tilloch.

Sir, Having recently directed my attention to the subject of refraction in azimuth, and having mentioned the subject to Mr. Williams, of Wells (at present residing in Islington), I have received the following letter, stating phænomena observed by him on the 28th ult. depending on azimuthal as well as vertical refraction. Similar effects were observed on the same day in the north environs of London, by Messrs. Whites of Finsbury-square, and myself; but being at that time in the prosecution of other inquiries, we could not attend particularly to the subject. Mr. W. however, between one and three o'clock, observed three sets of angles (in which the churches of St. Paul, St. Mary Islington, Hampstead, and Stoke Newington formed principal objects), without having any reason to suspect error arising from azimuthal refraction, the fog by this time being more uniformly dispersed, forming a general haziness in the horizon, but not reaching so high as the tops of the above spires.

I am, sir,

Your obedient servant,

26, Garlick-Hill, March 10, 1810. Jos. Steevens.

Mr. Steevens,—Sir,

Although I stated to you at our last interview, that in a very great number of observations, (probably a hundred,) made on Harrow, Hampstead, and another church lying S.W. of Primrose Hill, distance about four miles, I could discover no azimuthal refraction as you term it; yet I have since, viz. on Fast-day, observed it twice in a very striking manner, and must do you the justice, before I quit town, to say I am quite satisfied as to the fact.

On the above day about nine A.M., in my way to Kilburn crossing some high fields to the south of Primrose Hill, I observed, from the interposition of the fog, that several objects put on a strange distorted appearance, and that the spire of Islington church appeared crooked (see *first appearance*, Pl. VII); and in less than a minute, about 20 feet of the upper part appeared insulated and not immediately over the under part, but as in the margin. (See *second appearance*.)

I immediately screwed my telescope into the post of a fence which was near me; but before I could adjust it, the spire

spire became indistinct, being now wholly enveloped in the fog. I now directed the telescope to Hampstead (Harrow being invisible), the upper part of which was tolerably distinct, although there was a thick fog at the bottom of the hill. As soon as I had a tolerably good intersection, I quitted hold of the telescope, and found it remarkably steady, it being quite calm; I soon observed the tower to increase in its height, and shortly after the upper part appeared separated from the lower; the lower part retiring a little to the left, while the upper part was stationary in azimuth, but increased in altitude; in three or four minutes the top was considerably elevated, but still coincided with the vertical wire, and began to become indistinct as if rising into a cloud; but before it was quite obscured, I thought I discovered it to be a little to the left of the wire; the bottom part was now hazy, and still more to the left, apparently about $\frac{1}{6}$ of the whole diameter of the tower; in which situation it disappeared.

Islington spire was now just visible: it appeared upright, but as far as I could judge by intervening objects was much elevated. The telescope remained unaltered until the fog sufficiently cleared up for further observation on Hampstead, which was near three-quarters of an hour. I now found the top and bottom joined, and nearly in the same situation where the bottom appeared just before its obscuration, viz. considerably to the left of the wire, where it remained until the telescope was removed. It would thence appear, that when I first made the intersection the tower had acquired its greatest elongation, and perhaps might even then have been on its return.

I learnt from a gentleman in the course of the day, that one end of a row of houses near Holloway appeared to him first much higher and afterwards much lower than the other, although he did not change his situation:—and from another, that the top of Primrose Hill, with some persons on it, appeared to him separated from the bottom and floating in the air, and that he had seen a similar effect on other hills several times before.

I am, sir,

Your obedient servant,

Upper Street, Islington,
March 5, 1810.

CHARLES WILLIAMS.

XLVIII. *Second*

XLVIII. *Second Vindication of Dr. Herschel's Theory of Coloured Rings, in Answer to an anonymous Reviewer.*

To Mr. Tilloch.

Sir, The members of the club who formerly addressed you, have lately seen, in the twenty-first Number of the work called The Retrospect, some strictures on their vindication of Dr. Herschel's Essay on the Newtonian concentric coloured rings, which you honoured with a place in your Magazine for November last*.

Our main object in that communication, was to assert the validity of Dr. Herschel's important experiment described in the thirty-first article of his Essay. This experiment, the retrospectors, in their thirteenth number, attempted to set aside as nugatory, by affirming that the wedge of air described by Dr. Herchel was much too thick for exhibiting the coloured streaks which, according to the Newtonian doctrine of the fits of easy reflection and easy transmission of the rays, would have been seen in it, had it been sufficiently thin. In our former paper we showed, on the authority of sir Isaac Newton himself, that the retrospectors had asserted what was not just; and that Dr. Herschel's wedge, according to his measures minutely stated, was sufficiently thin for exhibiting the coloured streaks, if the Newtonian fits had a real existence; and that, according to this hypothesis, the author had a right to expect such coloured streaks; which failing, or not appearing, he had a right, as he contended for, to conclude that these fits are imaginary.

One proof concerning the competent thickness of his wedge being so complete, and so much held up to view by appearing in your excellent philosophical miscellany, the retrospectors have thought it necessary, as it would appear, to strike to it, by saying in number twenty-one, page 403, "We now come to the last and the most plausible objection that Dr. Herschel's friends have advanced to our remarks; and here we grant, that such a wedge as they have described ought to have produced the effects which Dr. Herschel expected from it." Now we must observe that the wedge of air we described was no other than the wedge described by Dr. Herschel: and what he expected from it, and what every body else must have expected, was this: namely, *coloured streaks*,—provided the Newtonian fits

* Philosophical Magazine, vol. xxxiv. p. 359.

fits really existed, but *no* coloured streaks if such fits were imaginary. The experiment was most carefully made, as appears by the thirty-first article, and no symptoms whatever of such coloured streaks were perceivable. Hence the author considered it as an *experimentum crucis*, disproving the reality of the Newtonian fits.

After this result of the experiment, and the admission of the retrospectors as above quoted, it might have been expected that they would no longer have resisted such evidence, or the just conclusion from it:—but no such thing; we now find them disputing the point as much as ever, by *new assertions* and contradictions, as totally groundless as that whose fallacy we have convicted them of. They immediately after the above quotation, now allege, that on account of corpuscular repulsions, &c., the wedge of air described by Dr. Herschel could not have been so thin as his measures, which before they did not challenge, show it to be; or so thin as to produce streaks according to the Newtonian doctrine of the fits: than which nothing can be more wide of the fact.

But, in another place, we find something prodigiously inconsistent with all this contention for extreme thinness of the wedge, before it is capable of showing streaks. In page 410, where the retrospectors want to set aside Dr. Herschel's explanation of the *bow-streaks* which he has shown to be occasioned by the application of a plain reflecting surface under the base of the prism, how do they do so? Still by resorting to the Newtonian doctrine of the fits. And now they would have these bow-streaks to be produced by the plate of air between the base of the prism and the reflecting surface applied to it, in consequence, they say, of the *great thickness* of this very plate, occasioned by their corpuscular repulsions. We shall say no more of such flagrant contradictions. In the same spirit, in the concluding paragraph, page 412, they deny that what Dr. Herschel calls the *critical separation* is capable of producing the bows; not perceiving that sir Isaac Newton has explained the formation of his blue bow on this very principle, as Dr. Herschel has explained his red bow.

In short, both attacks of the retrospectors abound with similar inconsistences and contradictions, which we think wholly undeserving of notice; as with such opponents we consider further discussions to be vain.

XLIX. *Report made to the French Institute on a Memoir of M.* DELAROCHE* *on the Air-bladder of Fishes.* BY G. CUVIER.

THE mathematical and physical class instructed Messrs. Lacepede, Vauquelin, and myself, to render an account of a memoir by M. Francis Delaroche on the air-bladder of fishes.

As several naturalists have been of late employed in directing their attention to the organ which is the object of this memoir, and to its functions, we do not think it will be improper to preface our report by a historical view of what has been said on the subject; a recapitulation for which M. Delaroche himself has furnished us with ample materials.

The air-vessel of fishes is too remarkable, it strikes the eye too forcibly on the first opening of the animal, and differs too much from every other organ, not to awaken the attention of naturalists; but, like most objects in comparative anatomy, it has long produced more conjectures and hypotheses than exact observations and experimental researches.

Rondelet * confined himself to the observation, that it existed more constantly in fresh than in salt-water fishes, and that it probably serves to assist them in swimming.

Marcus Aurelius Severinus risks an opinion that the air of this vessel was produced along with the animal; which proves that he had never perceived any communication with it outwards.

Gauthier Needham (in 1668) was the first who entered into more detailed inquiries, and inserted them in a book, where no one would expect to find them; namely, *De formato fœtu* †. Adopting the general idea of the utility of this bladder for swimming, he explained how flat fish are enabled to do without it; he described the two tunics of this organ, as well as the varieties of its form, and the origin of the canal of communication. He shows that the vessels are more abundant than are requisite for its own nutrition; that it is probable that some organic function is exercised by them, and that the blood contained in them has some connexion with the air: but judging that it would be difficult for the air to penetrate into it from without, in certain fishes, through substances which fill the

* *Hist. Pisc.* 1554, pp. 26 and 73.
† *Bibloth. de Manget.* ii. pp. 713 and 714.

 stomach

stomach, he conjectured that this fluid is secreted there, and that it proceeds from thence into the stomach, where it assists in the process of digestion: he even points out the red bodies which operate this secretion in the snake.

Borelli explained in detail, in 1676, the method in which the bladder is used in swimming. He observed that fishes, whose air-bladders burst, remain at the bottom of the water, as well as most of those which are naturally deprived of it; and concludes that it is intended to render the body of the fish sufficiently light to be in equilibrium with the water: he added, that by compressing the bladder, or by abandoning the air which it contains to its elasticity, the fish can augment or diminish its total specific gravity, and assist it in its ascent or descent. He supposed, that the canal which establishes in certain fishes a communication between the air-bladder and the stomach, must be a method of varying or renewing the quantity of air *.

To conclude: he has neither described the varieties of the structure of the bladder, nor determined in what fishes it exists, and those in which it is wanting.

Redi resumes the observations of Needham. He added some details on those fishes which have no air-bladder, and on the red bodies in the interior of several of these organs. He also stated, that he had in vain sought for the canal of communication in certain sea-fishes; but he thought that it was his fault, and this opinion of the generality of the existence of the canal has even reigned to the present time among some others. These remarks of Redi are still to be found in a book entitled, *Observations sur les Animaux vivans contenus dans les Animaux vivans*. Florence, 1684.

Ray and Willoughby, without making fresh inquiries, and without deciding on the manner in which the air is introduced into the bladder, disputed the idea of this air being used in digestion, and reduced the bladder to its employment in swimming, according to the ideas of Borelli. They insisted on the muscles peculiar to certain vessels, and mistook for them the red bodies in the interior of some others †.

The same opinion on the use of the bladder was supported by Preston ‡, by Perrault §, and by Petit ||. Perrault made the important observation, that there are fishes

* *De Mot. Animal.* cap. 23. *De Natatu.*
† Willoughby, *Hist. Pisc.* 1686, pp. 12 et seq.
‡ Phil. Trans. xix. p. 499.
§ *Mécanique des Animaux*, part II. ch. iii. vol ii. p. 383 of his works, 1721.
|| *Mem. de l'Acad.* 1733.

without

without any canal, and that it is in the latter that the red bodies are found, which are intended for the separation of the air. He added, that in those which have a canal, the air does not issue from the bladder, although it be compressed; a remark too much generalized.

Petit, on the contrary, thought he had discovered in the canal of the carp, valvuli which admit of the air escaping, but not of returning.

Notwithstanding the observation of Perrault, Artedi still ascribed to all bladders a canal destined, according to him, for the introduction of air: but, with the exception of Borelli's *, there is no opinion given respecting their use.

It is the same case with Gouan, Bloch, and a variety of other authors, who add nothing in other respects to the details previously acquired.

But, admitting in its fullest extent this chief employment of the air-bladder, we might still suppose it to have accessory uses, and in particular we must defer giving any opinion as to the origin of the air which it contains.

This was the conduct pursued by Vicq d'Azyr in 1773 ‡. He imagined that the air originated in the stomach, from whence it entered, charged with nutritive particles, into the air-bladder, in order to be absorbed by the vascular system. He was followed by Broussonnet in this idea § under some modifications.

Erxleben entertained the same idea respecting the propagation of the air; but as to its uses he followed the common opinion ‖.

These three anatomists seem to have been ignorant that the communication between the stomach and the air-bladder is frequently wanting.

This is strongly insisted upon by Kœhlreuter ¶ in an anatomical description of the lotus. After having ascertained the defect in the canal, and that a number of other fishes are also without it, and after having described the organization of the red bodies, he maintained that the air is separated from the blood in the bladder. He thought his system was new, not having read the writings of Perrault and Needham.

Leske adopted the opinion of Kœhlreuter **.

* *Partes Pisc.* 1738, p. 36. † *Hist. des Poissons*, 1770, p. 81.

‡ *Mémoires presentés*, tome vii.; and his Physiological Works, tome ii. p. 203.

§ *Var. Posit. circ. Respir.* sect. v.

‖ 1776, in a memoir on the subject; and also in his Natural History, edit. 1797, p. 279.

¶ *Nov. Comm. Petropolit.* tome xix. 1775. ** *Hist. Nat.* 1724, p. 390.

 Monro,

Monro, who in his work on fishes ought to have thrown a great deal of light on this subject, has added but little to what was known before on this subject. He made the same distinction with Perrault between bladders with secretory red bodies which have no canal, and those which have a canal and want these bodies; but he does not mention any French anatomist; perhaps because he had never read any of their works on the subject.

He remarked that the genus anguilla formed an exception to the rule, from having the canal and red bodies. With respect to the other parts of the question, he did not decide upon the use of the bladder; and merely inquired, if fishes could not, in swallowing, distinguish the bubbles of air from the mass of water, and make them pass in preference into this organ.

M. Fischer, now professor at Moscow, published in 1795, at Leipsic, a particular dissertation on this subject; in which after having given an extract of the writings of his predecessors, and having communicated his own observations on the carp and the tench, he hazarded the opinion, that the air-bladder, independent of its uses for motion, is also a supplementary organ of respiration, destined to absorb the oxygen from the atmospheric air contained in water, as the gills are destined, according to him, to absorb the oxygen of the water itself, by decomposing it.

M. de Lacepede supposes, that certain fishes may at least fill their bladder with the gases resulting from the decompositions which their respiration occasions. He thought that it was frequently hydrogen with which it was filled, and he mentioned tenches in which he had collected precisely this kind of gas.

Finally, M. Duvernoy, editor of that part of Cuvier's comparative anatomy which has for its object the air-bladder of fishes, adopted, in common with M. Cuvier, the opinion of Needham and Kœhlreuter, that the air is produced in the bladder by secretion. He also described some of the organs of this secretion in fishes not before observed; but, from too much precipitation, he forgot to advance the principal argument, founded on the absence of all canal of communication in many species. He concludes, from the absence of the vessel itself in fishes belonging indiscriminately to all descriptions of families, and even to genera the other species of which are furnished with it, that its functions cannot be very essential to life. By comparing its proportional volume with the nature of the movements of every fish, and by examining the supplementary means granted

granted to those who have it not, and the various effects of those means, he arrives at the conclusion that it is essentially an organ connected with loco-motion.

He expresses his astonishment at the discordance between the analysis hitherto given of the air contained in this bladder; some, like M. Fourcroy, having found hardly any thing but azote; others, like M. Configliati, having found so much as 40·0 of oxygen; while others, like Mr. Broadbelt, found the quantity variable in the same kind of fish according to circumstances. M. Duvernoy concludes with suggesting that chemists should inquire into the causes and limits of these variations; a precise knowledge of which could alone decide a great number of the questions in dispute.

Messrs. Geoffroy and Vauquelin on one hand, and M. Biot on the other, have recently made a great part of the experiments which were pointed out as requisite by M. Duvernoy.

M. Biot, in his first voyage to Ivica, examined the air in the bladder of several fishes of the Mediterranean, and found that it varied from pure azote up to 87·0 of oxygen, with very little carbonic acid, and without any hydrogen, and that in general the oxygen is the more abundant, in comparison to the azote, as the fish comes from a greater depth, although the water at these great depths does not contain a purer air than that which is at the surface.

He also made the curious observation, that in fishes suddenly drawn from a great depth, the air-bladder ceasing to be compressed by the enormous column of water which bore upon it, is dilated so suddenly that it tears the intestines, and is ejected from the mouth. As to the origin of the air contained in it, he seems to think it has been secreted.

The experiments of Messrs. Vauquelin and Geoffroy, published by M. Biot, confirm his own on the subject, so far as the fishes on which they were made, living in our fresh waters and at very small depths, gave but very little oxygen. They agree also with other more ancient experiments of M. Fourcroy, who had found nothing in the bladder of the carp but azote almost pure, and with the analysis made by M. Humboldt of the air in the bladder of the gymnotus electricus, which consisted of 96·0 of azote and 40·0 of oxygen.

Such was the whole of our knowledge of the air-bladder of fishes when M. Delaroche read his memoir to the Institute. But in order to complete the series of facts which are

 necessary

necessary to guide us in forming an opinion of his theory, we think it right to say a few words upon two memoirs published since.

One of these, by M. Geoffroy, refers to an earlier memoir, in which he develops, anatomically, the means by which the fish compresses or relaxes its bladder, in order to descend or ascend. Indeed, he says at the same time, in the introduction of his memoir, that the bladder is by no means an organ of motion by itself; but this is because he thought that those who regarded it as such, suppose that it is dilated by the increase of the air which it contains, and *vice versâ*, an opinion which no person seems to have entertained; for it is always by the action of the muscles that it has been made to be compressed or dilated: on this subject, therefore, M. Geoffroy is really of the opinion of Borelli, which is the commonly received idea.

The other memoir to which we have alluded, is by Messrs. Humboldt and Provençal, and has for its chief object the respiration of fishes; but these authors have naturally been led to examine the air in the swimming-bladder.

They operated upon river fishes, and found the air variable in composition from 99·0 of azote to 87·0. They have observed as much as 5·0 of carbonic acid. They made some tench respire hydrogen, and yet their air-bladders when examined exhibited none: by keeping them in oxygen, however, the proportion of the oxygen in the bladder was somewhat increased. On removing the bladder from them, they were not prevented from producing by their respiration the ordinary effects upon the atmosphere; they were even able to raise themselves in the water, although they generally remained at the bottom of the vessel.

Thus, in the numerous works we have analysed, almost every possible hypothesis has been proposed, attacked, or defended, and examples have been given of almost all the combinations of organization that could be devised. M. Delaroche had only therefore to examine these organizations a little further, in order to reduce them to general rules, and to weigh over again the arguments advanced for or against every hypothesis.

Let us see how he has acquitted himself of this task.—

His residence at Ivica, Formentero, and on the coast of Spain, with Messrs. Biot and Arrago, having furnished him with opportunities of examining a great number of Mediterranean fishes not to be seen any where else, and their air-bladders having chiefly occupied his attention, he continued his inquiries after his return, on our common fresh-

fresh- and salt-water fishes: hence he has furnished upwards of fifty particular descriptions of the air-bladders of as many species of fish, several of which have not hitherto been described. These descriptions, added to those which former authors had given of some species which M. Delaroche could not find, form the materials of his present memoir; and he has placed his own at the end of the work, as so many proofs of the general propositions which he lays down.

In the body of the memoir he treats successively of the anatomical structure of the air-bladder, of the nature of the sources of the air which it contains, and of the functions which it exercises.

He speaks in the first place of its existence, and gives a list of those fishes which have it, and of those in which it is wanting. The results of this list, which adds several species to those which had already been adduced with respect to this subject, are nearly the same which had been already drawn; namely, that the existence or non-existence of the bladder does not correspond with the other affinities of organization which connect fishes with each other.

He afterwards speaks of the various situations of the bladder, of its variation in size, and in the configuration of its tunics, (an article in which he compares the internal membrane to the serous membranes); and finally, of the particular muscles which it has in several fishes; and he gives a more detailed description of these muscles than is to be found in the comparative anatomy of M. Cuvier.

What he says on the subject of the canal of communication also presents a great number of novelties. On this head he has made some very acute remarks, and has ascertained that this canal is wanting in the greater part of sea fishes. He did not find it in any of the jugular or thoracic classes, which compose nearly three-fourths of the total species of fishes with which we are acquainted. The lectures on comparative anatomy had assigned this canal to the *uranoscope*, which is a jugular; but according to M. Delaroche, the authors of this work have made new inquiries, and found that they were mistaken.

M. Delaroche has studied in a particular manner the red bodies with which certain bladders are furnished. He found them, like Perrault and Monro, in all those which want the canal of communication, and in the anguilla genus although furnished with this canal.

Our author gives a very detailed description of these bodies, in the *gadi*, the *trigli*, the perches, some *labri* and *holocentres*, as well as in the *atherina rhepsetus*, the *blennius physis*,

physis, the *orphius* or *esox belonus*, and lastly in the eel and the conger.

We have verified that part of the descriptions which refers to the species with which we are familiar, or could procure, and have found them generally correct.

It appears to us, however, that M. Delaroche grants too great a homogeneity to the inner texture of these bodies. One of our number, who, along with M. Duvernoy, recently made some inquiries in order to verify this point of anatomy, found these bodies in the larger fishes formed of lobes flattened like ribands, placed almost parallel on each other, very distinct from one another by clearly marked intervals, and proceeding obliquely in various directions from the proper membrane to the internal membrane of the bladder.

The distribution given by M. Delaroche of the vessels which issue from the red bodies of the eel, and from those which return to it, has also been verified, and found correct; but he passes rather too hastily over the red body itself, which is also divided into flakes, separated by intervals, which are frequently found filled with blood.

In short, Messrs. Cuvier and Duvernoy think they have found strongly marked relations in the red bodies of fishes with the cavernous bodies; but their inquiries posterior to the memoir of M. Delaroche, are only brought forward here that the Institute may not be ignorant of what has been done on this interesting subject. A full account of their experiments will shortly appear.

The author of the present Memoir speaks only from the lectures in comparative anatomy as to certain branching air-bladders, entirely peculiar to one species of fish. M. Cuvier, who had described them when on the sea coast, where he had no books from which to determine the species of the fish, thought it was the *perca labrax*; but other naturalists, besides himself, have since sought for it in vain in the fish so called in the systems of ichthyology. By unexpected good fortune, the true fish which was the subject of observation was brought to Paris some time ago, and proved to belong to the rare species denominated by M. Lacepede *cheilodiptera*, or sea eaglet, but which ought to be placed among the centropommes, beside the *labrax*.

The bladder of this fish, unique of its kind, will be presented to the Institute along with a description by M. Duvernoy, and which will be more minute than any hitherto given under less advantageous circumstances.

In his analysis of the air contained in the bladder, M. Delaroche

Delaroche confirms in general the experiments of M. Biot; adding, that besides the various degrees of depth at which fishes live, there are other causes which concur to vary the proportions of the gases in their air-bladders. Thus, of two fishes caught on the same spot, one has given 50·0 and the other scarcely 40 of oxygen. M. Delaroche also rectifies the idea that M. Biot had given of the eruption of the bladder from the mouth, in fishes drawn up suddenly from great depths, when he says that a rupture of the bladder then takes place, and that it is the air which forces up the stomach to the mouth. As to the source of this air, our author (like Needham, Perrault, Monro, Kœhlreuter, Duvernoy and Cuvier,) thinks it is produced in the interior of the bladder by a secretion of an unknown nature, of which the red bodies seem to be the organs in such fishes as have these bodies.

It is unnecessary to ask for a proof of this opinion in fishes which have no exterior canal, for in them it is demonstrated by itself. We might also fairly extend it to those which have a canal and red bodies, like the eel.

But in those which want the red bodies, as we must admit a new kind of exhalation, the analogy no longer takes place completely; and perhaps many persons would be equally willing to have recourse to the aërial canal, in so much as it always exists in this description of fishes. As fishes of the same family frequently have the air-bladder, and others want it, it is probable that its functions may be supplied by different means.

M. Delaroche, without considering that question as at all decided, nevertheless supports the argument of analogy, from the difficulty which any given gas would have in many species, in penetrating into the bladder by the canal; from the still greater difficulty which it would have of arriving pure, particularly when it was requisite for it to pass through the substances contained in the stomach; and, lastly, from the difficulty of knowing, from whence, or by what mechanism, the fish could procure it from nature, in order to introduce it into its bladder at great depths, where it is so frequently and so long retained.

The habit in which physiologists are of seeing matters of every kind come out of the blood by secretions, renders them on the contrary very easy as to this kind of production; and in fact there is no real difficulty on the subject, since azote and oxygen, which compose the air in the bladder, exist abundantly in the blood.

But it may be asked; If the gas be exhaled or separated from

from the blood, wherefore does it vary so much when the greater part of the other secretions are so constant in their nature? Above all, how can the animal body, so greedy of oxygen in general, exhale it so precisely at depths where it has the fewest methods of getting it from the external medium? M. Delaroche, who puts these questions, admits that it is difficult to answer them satisfactorily.

He afterwards proceeds to the uses of the air-bladder.

From its absence in many fishes taken indiscriminately from all classes, he concludes, with the authors of the comparative anatomy, that it cannot hold an important place in the vital functions; and this makes him reject all necessary connexion between the air-bladder and respiration.

He would have even been inclined to conclude, from its solute stoppage in the greater number of fishes that are furnished with it, that it could not in general be employed in the absorption of any useful matter, in the excretion of any injurious substance, nor even in the production of a substance to be employed in some other part of the body; but that it is solely by itself as the air-vessel, and in its quality of considerable capacity, filled with a light elastic substance that it may be useful to the fish.

Now in this respect it can only have a mechanical use, either with respect to its station or movement.

M. Delaroche in the first place ascertains its use in the station, and admits that it serves to render the whole fish specifically lighter, and to place it in equilibrium with the water in which it is suspended.

This is one part of the most generally received opinion; but it is clear that the necessity of the bladder for this sole purpose is any thing but demonstrated. Nature would rather have made all fishes of the same gravity as the water, as she has done with those fishes that have no bladders: thus, the common opinion is also composed of two other integrant parts equally necessary with the former. The one is, that the fish can compress as it pleases, to a certain extent, its bladder, or dilate it; which we prove by the peculiar muscles with which the bladder is furnished in certain fishes, and by the mediate action which the sides and the muscles of the abdomen exercise on it in all those which have it.

M. Delaroche also adopts this second part of the common opinion.

He thinks even that it is in this way the fish supplies, when it rises, the pressure exercised on its bladder in deep water by the column of water above it. Were it otherwise, the

the air, which would be no longer compressed, would be too much dilated, and would render the fish too light, or even produce some rupture, as happens to fishes drawn suddenly from great depths.

But who is there who is not aware, that this, on the part of nature, would be correcting very clumsily a defect which she might have refrained from introducing at all into her work? She had only to give no air bladder at all to fishes; and we have seen that she need not have done so to place them in equilibrium with the water: in that case she would no longer have required the apparatus of compression, which has been supposed as serving only to correct the inconvenience of an useless bladder.

Thus we are of opinion that the third, and the chief part of the commonly received opinion, in reality resolves the problem: we mean that part of it which says, that the bladder is placed there to assist the fish in ascending and descending, according as it is compressed and dilated; and we confess that we do not see why M. Delaroche should reject this use of the bladder, to which the two others are, in our opinion, merely accessaries.

That the fish has strength sufficient to enable it to descend, clearly results from what M. Delaroche himself admits; for if the fish, which ascends 30 feet for example (and it is difficult not to believe that many fishes can ascend that height without any accident), if, we say, such a fish has sufficient strength to compress its bladder, by means of its muscles, to the same degree that the 30 feet of water formerly did, it is evident that a similar fish, supposed to be in equilibrium at the height to which the former ascended, will also have sufficient strength to compress its bladder, as much as would the addition of a weight of 30 feet, and that there would result from such a compression or diminution of volume more than sufficient to force it to descend.

M. Delaroche, against this most essential part of the vulgar opinion, advances only a single objection, which he borrows from M. Fischer: this is, that the variation of specific gravity which may result, with respect to the total body of fishes, from the variations of the volume of the bladder being very small, the ascents or descents, which are the consequence of it, could not but be very slow: but, besides, the circumstance of these variations never having been yet measured, no person has ever said that the bladder cannot be aided in this function by other organs. Those fishes which have no bladder, ascend and

descend

descend very well, although, other circumstances considered, rather more tardily. Now those which have a bladder have, in addition, all the organs employed by those which have none, and they can use them like the others.

One difficulty which we have sometimes heard started, is to ask how a fish, when it wishes to ascend from the bottom of the sea, can find strength to raise up by means of its sides, or generally of its integuments, the enormous column of water which lies upon it, in order to permit its bladder to dilate. But as this vessel is already, by its compression, in equilibrium with the water which presses it, the least effort is sufficient; and even this effort, however small, is nevertheless necessary, that the fish may only rise a few feet by the means which are common to it with the fishes without bladder: instantly its bladder, being less compressed, will be too much dilated; and, according to M. Delaroche's own experiments, it will carry it precipitately upwards, and burst its entrails if it does not speedily close it. This second objection is refuted therefore like the former.

Thus we think we ought to adhere to the ideas of Borelli, as to the use of the air-vessel of fishes; but although we differ in opinion from M. Delaroche on this point, we do not the less regard his Memoir as worthy of approbation, from the great number of new and correct observations which it contains, as to the anatomical structure of the bladder, as well as upon the nature and sources of the air which it contains; and we have the honour to recommend that it should be printed among the Memoirs of *Savans Etrangers*. Signed,

LACEPEDE, VAUQUELIN, CUVIER.

L. *Notices respecting New Books.*

MR. ACCUM has just published a "*Manual of a Course of Lectures on Experimental Chemistry and on Mineralogy; containing, in the Form of Axioms and concise Outlines, the Elements and fundamental Truths of chemical and mineralogical Science: intended to illustrate the Lectures on these Branches of Knowledge in the Theatre of the Surry Institution; with an Account of the Action of chemical Tests and Modes of applying them in the Practice of the Science.*"

THE very full title of this little work (about 230 pages small 12mo.) sufficiently explains its object. The author states that in writing it he has endeavoured to exhibit, to the learner, those "important facts and fundamental doctrines

trines which constitute the theory of the science. The laws of chemical action, which befall all bodies comprehended under the chemical domain of nature, have been detailed in the synthetic form of propositions—the natural history of the most characteristic bodies stated—the processes employed in the laboratory for obtaining them, described—and their physical and chemical attributes pointed out, so as to interest the mind, and fix the doctrines in the memory."

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

MARCH 28.—The President in the chair. A letter from Mr. Groombridge to the Astronomer Royal was read on the refraction of light. The author determines the mean astronomical refractions from the observations of fifty circumpolar stars; as also from the zenith distance of the sun at the solstices. The quantity he assumes at 45° is $56\frac{1}{2}''$; and this is corrected by the difference of the errors of the co-latitude thence found. The mean refraction at 45° the author states to be $58\cdot107'' \times$ tang. zenith distance, $-\ 3$ times the refraction. He then corrects the formula of Bradley, by comparing the mean refraction of the pole star with stars at low altitudes; whence he determines the refraction from the zenith to the horizon to be $58\cdot119'' \times \text{tang}\ \overline{z - 3\cdot36\,r}$, and which he shows to agree with the new French tables, by a more simple formula than that of M. de la Place. The author also proposes a correction for the thermometer, drawn from his observations; something differing from that of Dr. Bradley.

April 4.—A letter from Mr. Brinckley of Dublin to the Astronomer Royal was read, stating the discovery of the parallax of the annual orbit of 13 circumpolar stars, which he has found to be $2\frac{1}{2}$ seconds: he also ascertained their refraction somewhat similar to that of Mr. Groombridge. The letter concluded by saying, that the writer is pursuing his researches in order to verify the discovery here announced.

Some observations on the gizzards of swans, geese, and herbivorous fowls, compared with those of turkeys, were communicated by Mr. Home. They consisted of a brief description of the process of mastication and digestion in ruminating animals, by chewing their food slightly, then swallowing it afterwards, bringing it up in round balls, mixing

mixing them with saliva, and finally passing them to the fourth stomach, there to be digested. The process of digestion in geese and swans is extremely slow, as it is effected by the action solely of the muscles of their stomachs. Mr. Home examined the structure of their stomachs by filling them with plaister of Paris, and boiling them; when they appeared composed of straight muscles united by filaments, as observed by Spallanzani.

April 11.—A part of a curious paper by Mr. Macartney on luminous animals was read. Mr. M. took a brief review of the different creatures in the animal kingdom which emit light, whether in or out of the sea, as the lampyris, fulgora, &c. He examined minutely the assertions of some French naturalists, who maintain that common earth worms have occasionally appeared luminous, and proved that they are unfounded. The luminous appearances in the sea, which have been so often noticed, and yet so imperfectly explained, were next discussed; and also how far it is probable that the emission of light depends on the will of the insect; and whether nature has given this faculty to females in order to attract the males to them, as has been alleged.

The reading of the remainder of this paper was postponed till next meeting, and the society adjourned till the 3d of May.

SOCIETY OF ANTIQUARIES.

Monday, April 23, being St. George's Day, the Society of Antiquaries met at their apartments in Somerset-place, in pursuance of their statutes and charter of incorporation, to elect a president, council, and officers, of the society for the year ensuing: whereupon

The most noble George, marquis of Townshend and earl of Leicester,

F. A. Barnard, esq.	William Hamilton, esq.
W. Bray, esq.	Samuel Lysons, esq.
Nich. Carlisle, esq.	Craven Ord, esq.
Sir H. C. Englefield, bart.	Matthew Raper, esq.
Anthony Hamilton, D. D.	Joseph Windham, esq.

Eleven of the council were re-chosen of the new council: and

Charles Baratty, esq.	Edward Jerningham, esq.
Right Hon. P. R. Carew,	Charles duke of Norfolk,
William, lord bishop of Cloyne,	John Towneley, esq.
Henry Ellis, esq.	Brig.-gen. T. H. Turner,
Henry viscount Harberton,	Rev. Stephen Weston,

Ten

Ten of the other members of the society, were chosen of the new council, and they were severally declared to be the council for the year ensuing; and on a report made of the officers of the society, it appeared that the most noble George marquis of Townshend and earl of Leicester was elected president, William Bray, esq., treasurer, Matthew Raper, esq, director, rev. T. W. Wrighte, A. M. secretary, and Nicholas Carlisle, esq. secretary for the year ensuing.

The society afterwards dined together at the Crown and Anchor tavern in the Strand, according to annual custom.

SOCIETY FOR THE ENCOURAGEMENT OF ARTS, &c.

At a late meeting of this society a premium of fifty guineas was awarded to Mr. John Davis of John Street, Spital Fields, for a most ingenious fire-escape, which promises to be of singular use in lessening the number of personal accidents which occur so frequently in this great city in cases of fire. This contrivance consists of a most curious yet simply constructed ladder, or rather three ladders so combined as to admit of their being slid out, like the tubes of a pocket telescope, to the height of from forty to fifty feet if required; carrying up at the same time a box to receive females, or children, or small valuables, (while the less timid can descend by the ladder). This box, by means of a chain and pulley worked by the people below, descends to the ground; where being instantly unhooked, another box is sent up while the first is emptying. All this is performed in about two minutes. This apparatus is erected on a carriage with four wheels, 9 feet long and 5 feet wide, furnished with the usual apparatus and harness for yoking a horse to it, for the more speedy removal to the scene of danger.

The fire offices, much to their credit, assisted the inventor with money to construct a machine of this kind for real use, after having inspected his model. We have examined the machine, witnessed the facility with which it performs the destined operation, and we have no hesitation in recommending to every parish in London to provide one of them to be kept with their fire ladders and engines.

This new fire-escape may be seen at Mr. John Bevan's, carpenter, City Road, near Finsbury Square.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this society on the 10th March, the rev. Dr. Macknight read a paper on the mineralogy of

 Strontian

Strontian and Ben Nevis. The rocks which compose the districts of Strontian are mica slate, gneiss, and granite; and the lead-glance, which occurs in gneiss, is associated with iron pyrites, cross-stone, calc-spar, foliated zeolite, strontian, and heavy spar. Ben Nevis is an overlying massive formation, which rests on gneiss and mica-slate, approaching in some places to clay-slate. In this formation compact feldspar is the leading ingredient. The inferior mass consists of sienite, passing from the simple granular to the granular porphyritic; and the upper portion of the mountain, comprehending the summit, with about 1400 feet of the perpendicular height below it, is composed of a dark-coloured rock, which, for the most part, is porphyritic, and seems to be intimately allied in its characters to compact feldspar. This appears from the gradual transition of the one substance into the other, which is distinctly observed under the tremendous precipice of Ben Nevis to the NE. and demonstrates the identity and continuity of the whole formation. The colouring matter appears to be hornblende intimately mixed with the substance of the rock. At first view, the whole mass might be considered as a formation of clinkstone and porphyry-slate. But a more minute investigation discovers many oryctognostic characters of distinction from these substances, which are less crystalline, and belong to a more recent æra of formation.

At the same meeting, Dr. Arthur Edmonstone read an account of the peculiarities of the Zetland sheep; with remarks on their diseases. And the secretary read a communication from lieutenant-colonel Imrie, describing a vein of greenstone, which occurs in Glencoe, and which appears to have been overlooked in the mineralogical descriptions of that district.

FRENCH NATIONAL INSTITUTE.

*Analysis of the Labours of the Class of Mathematical and Physical Sciences of the French Institute, for the Year 1809**.

All the sciences which are founded upon facts have this distinguished advantage, namely, that every experiment and every observation contribute to their progress. Indeed, properly speaking, there are no discoveries made in vain, so far as the physical sciences are concerned. Whatever may

* Translated from the original, distributed at the public sitting of the class, 2d Jan. 1810.

may be the consequences to which they lead, whatever are the results obtained, provided they be just, the instant they assume a character of novelty, they become useful: every fact has a determinate place, which can be held by itself alone, and we ought to consider the edifice of the sciences as that of nature: every thing is infinite, every thing necessary. We may go further: in short, the progress of truth is not essentially retarded because those who devote their talents to philosophical subjects occasionally fall into erroneous roads. The most useful discoveries have sprung from the greatest errors. We find the proof of this in the labours which have been undertaken to overturn modern chemistry, and to support the old theory of combustion. The complication of the phænomena in this latter science will even be the cause that the proofs of this description will still continue to be multiplied: facts do not always present themselves under the same characters; they are studied under other points of view, they are seen with different eyes, and the results to which they lead are not similar. This is precisely the case at present with respect to the discussions which have arisen between Mr. Davy and associates Messrs. Gay Lussac and Thenard.

CHEMISTRY.

In former reports we have given an account of the discovery of Mr. Davy, as to the changes that potash and soda undergo by the action of the Voltaic pile, and of the processes by which Messrs. Gay Lussac and Thenard produced these changes without the help of the above instrument.

Mr. Davy thought that in these experiments the potash and soda were subjected to a deoxygenation, and that a true metal resulted from it, which was particularly distinguished from other substances of this kind by an extreme affinity for oxygen. He called these two metals *potassium* and *sodium*. Messrs. Gay Lussac and Thenard, established on the contrary, by several experiments, but particularly by the products obtained on analysing the combination of the potassium with ammonia, were of opinion that the changes of potash and of soda were owing to a particular combination of these alkalis with hydrogen. Mr. Davy, having repeated the experiments on which this opinion is founded, has not obtained results conformable to those which had been announced by the French chemists: this has given rise to some observations by Messrs. Gay Lussac and Thenard, in which they show that the differences found between the results of Mr. Davy's experi-

ments and their own, appertain to causes which cannot influence the consequences to which they have led.

To conclude:—On either hypothesis, the discovery of Mr. Davy has produced an extremely active re-agent in chemistry, and which must produce, on other bodies, effects hitherto unknown.

This new discovery gave rise, therefore, to very different experiments, but which led to the same end: some had for their objects to ascertain the action of the pile on the other alkalis, on the earths, and generally on all the simple non-metallic substances which we might suppose to be oxides, like potash and soda. The object of the others was to decompose, by means of the new metals, substances oxygenized, or supposed to be such, and particularly the boracic, fluoric, and muriatic acids.

We informed the public last year, that Messrs. Gay Lussac and Thenard had succeeded in effecting the decomposition of the first of these acids, and had ascertained its radical. Since that time their inquiries have been directed to the fluoric acid.

They began by studying the physical and chemical properties of this acid more precisely than had been done by any one else. The affinity of water for this gas is extreme: as soon as it is mixed with other gases which contain some portions of this liquid, abundance of vapours is formed: nevertheless, this gas cannot communicate its expansive force to water; it cannot be dissolved nor *gazify* the smallest quantity, and in its aëriform state it is absolutely dry: but it is impossible to obtain this acid pure; it always retains some portions of those bodies with which it has been in contact; and in the labours which Messrs. Gay Lussac and Thenard have undertaken on the subject of this acid by means of potassium they made use of siliceous fluoric gas in preference, as not containing any foreign body susceptible of being decomposed and obscuring the results of the experiments. In the reciprocal action of these two substances, there is a great absorption of fluoric acid, very little hydrogen gas extricated, and a transformation of the metal into a solid matter the colour of which is reddish brown.

Messrs. Gay Lussac and Thenard regard this new combination as a compound of potash, of silex, and of the radical of the fluoric acid; but they have not been able to obtain this last substance in a separate state. "It appears," say they, (after several experiments which we cannot give here,) "that when this radical is combined with potash only,

only, it may decompose water like the phosphures; but when it is combined with potash and silex, it does not decompose it: doubtless, because this triple combination is insoluble."

Mr. Davy has also made attempts to obtain in a free state the fluoric radical, and has obtained results analogous to those above referred to: he ascribes the hydrogen produced in the combination of the potassium with the gas, to the water which he thought was contained in this acid, and which the metal had decomposed.

The muriatic acid has also been the subject of numerous and interesting experiments by Mr. Davy and Messrs. Gay Lussac and Thenard. All three have made some fruitless attempts to decompose this acid, and to insulate the radical which has been considered as forming one of its elements. But Messrs. Gay Lussac and Thenard have ascertained that the muriatic acid could not exist without water in the state of gas; that it then contains one fourth of its weight; and that water alone possessed the property of taking it up from its dry combinations. It must be remarked, that in all the experiments made with the metals, the water, on being decomposed, has always produced a quantity of oxide equal to what was required by the acid, in order to neutralize it; so that, for every result, hydrogen and a neutral salt were obtained. The limits of this report do not admit of our detailing all the experiments made by Messrs. Gay Lussac and Thenard; but we ought not to pass over the happy application which these chemists have made, in the decompositions of the muriate of soda, of the affinity which the muriatic acid has for water. We know that soda enters as a primary matter into several branches of manufacture, and it is very important to possess a simple and direct method of extracting this alkali from common salt.

As to the oxygenized muriatic acid, Messrs. Gay Lussac and Thenard subjected it to numerous experiments:— "These," they inform us, "ought to give an idea of the constitution of this acid totally different from that which has been formed. It has been regarded as the most easily decomposed body, and on the contrary it resists the action of the most energetic agents. We cannot extract the muriatic acid from it in the state of gas, except by means of water or hydrogen." This acid weighs 2·47 more than air. It contains the half of its volume of oxygen gas, and all the water which it can form with the hydrogen is retained by the muriatic acid which it contains. This water makes one fourth of the weight of this last acid.

The action of the metal of the potash on the oxides and the metallic salts, and on the earthy and alkaline salts, has also been particularly examined by Messrs. Gay Lussac and Thenard. It results from their inquiries, that all the bodies in which we know the presence of oxygen are decomposed by this metal; that this decomposition almost always takes place with an extrication of light and heat; that this extrication is the more considerable in proportion as the oxygen is less condensed; and that, consequently, this might furnish a method of appreciating the degree of condensation of the oxygen in any body.

After having operated on potash and soda, by means of the Voltaic pile, the charges mentioned in the early part of this memoir, it was natural to endeavour to produce analogous effects on the other alkalis, and on the earths. In short, Mr. Davy undertook numerous experiments, in order to discover, according to his system, the metals of barytes, strontian, lime, magnesia, silex, alumine, zircon, and glucine. After several fruitless efforts, he succeeded in de-oxygenizing the first four of these substances, and in forming amalgams of the new metals which resulted. He thinks that the other four are also metallic oxides; but his experiments, as he confesses, do not prove this in a satisfactory manner.

Another amalgam produced by ammonia was discovered last year, at Jena, by Dr. Seebeck. This afterwards became the subject of certain researches by Messrs. Berzelius and Pontin of Stockholm, and by Mr. Davy in England: all three agree in ascertaining ammonia to be a metal. In the ordinary temperature of the atmosphere this amalgam has the consistence of butter, and in the cold it crystallizes in cubes; but the new metal has not yet been obtained in a separate state. Messrs. Gay Lussac and Thenard have repeated and proved the correctness of the above experiments. But this amalgam, which had been formed by the action of the pile only, has been produced by the French chemists by the action of the metal of potash, and they have ascertained that a slight agitation was sufficient to decompose it. By this simple action the mercury once more becomes fluid, and ammonia and hydrogen are liberated in the proportion of 28 to 23. The mercury absorbs 141 times its volume of hydrogen gas, and 88 times its volume of ammoniacal gas, in order to pass to the state of amalgam: whence it results, according to our authors, that in this combination the mercury increases about 0·0007 of its weight; whereas, according to Mr.

Mr. Davy, it should only increase one 12000th part. Thus the theory by which Messrs. Gay Lussac and Thenard explain the formation of potassium may be applied to the formation of ammonium. This new metal, according to them, is nothing but ammonia and hydrogen.

Finally, Mr. Davy has also directed his attention to sulphur, phosphorus, plumbago, charcoal, and the diamond. The chief experiments relative to these two first substances have been made on hydrogen, sulphuretted and phosphuretted gases, by means of potassium; and he concludes, from the results obtained by him, that these two inflammable bodies are combinations of hydrogen, oxygen, and an unknown base, and which has not yet been obtained in a separate state. As to the other substances, he is inclined to regard plumbago as an alloy of iron with a peculiar metal which is found in charcoal combined with hydrogen, and in the diamond with a small part of oxygen.

These ideas were too strongly contradictory of those which are commonly received, not to excite the inquiries of other chemists. Messrs. Gay Lussac and Thenard therefore made sulphur and phosphorus the subjects of a very extensive series of experiments; and as Mr. Davy had employed the hydrures in his experiments, the French chemists in the first place endeavoured to determine the elements of these substances with precision. They ascertained that sulphuretted hydrogen gas contains a volume of hydrogen equal to his quantity; that the phosphuretted hydrogen gas contains at least one and a half of its volume; that the former of these gases may be absorbed by potassium and sodium; and that in this absorption there is developed precisely the same quantity of hydrogen which the metal alone would give with ammonia and with water: lastly, that phosphuretted hydrogen gas is decomposed by potassium and sodium, so that the phosphorus is combined with this metal, and the hydrogen is set free. But these chemists have not confined their researches to the substances employed by Mr. Davy: they made experiments on arseniated hydrogen gas, and found that this gas acts with the new metals in the same way with phosphuretted hydrogen gas; and that the metallic arsenic may be combined with hydrogen so as to form a solid hydruret, which has the form of light flakes of a brown colour. They concluded that sulphuretted and phosphuretted hydrogen gas, as well as sulphur and phosphorus, contain no oxygen, or at least that the experiments of Mr. Davy do not demonstrate it. They are of opinion, however, as has been already

ready suggested, that sulphur, and perhaps phosphorus, contains hydrogen.

We shall not presume to decide between the opinions of Mr. Davy and of Messrs. Gay Lussac and Thenard; but it will not fail to be remarked, although this cannot lead to any consequence injurious to modern chemistry, that hydrogen, which frequently in the theory of Stahl was nothing else than phlogiston, produces combinations which have all the characters of the metals.

In addition to the labours we have mentioned, we are indebted to M. Gay Lussac for some observations on the combination of gaseous substances with each other, which led him to prove that the gases, in such proportions as to render them fit for combination, always produce compounds the elements of which are in very simple ratios with each other. Thus, 100 parts of oxygen gas saturate exactly 200 parts of hydrogen; the fluoric and muriatic gases, mixed with the ammoniacal gas, saturate a volume of the latter equal to their own, and form neutral salts, &c. But he observes, that, when we consider the proportions in weight, we obtain no simple ratio between the elements of a similar combination. Moreover, he shows that the apparent contractions which the gases undergo on combining, also form very simple ratios with the primitive volume of the gases, or only with the volume of one of them; and he afterwards makes the remark, that the apparent contraction does not indicate the real contraction which the elements have undergone in combining.

These observations have been followed up by a particular inquiry as to the nitrous vapour and nitrous gas considered as a eudiometrical method. Here we see in a very evident manner the influence of the quantities on the result of the combinations. If we mix 200 parts of nitrous gas with 200 parts of oxygen gas, nitric acid is produced; and 100 parts of oxygen remain at liberty. If, on the contrary, we mix 100 parts of oxygen and 400 of nitrous gas, an absorption of 400 parts takes place, which produces nitrous acid, and 100 parts of nitrous gas remain free. Thus we obtain nitric acid, or nitrous acid, according as either of the gases of which these acids are composed is predominant.

But in both cases the absorptions are always constant. Thus, the nitric acid is composed of 100 parts of azotic gas and 200 of oxygen gas, or 100 of oxygen gas and 200 nitrous gas. The nitrous acid results from the combination of 100 parts of oxygen gas and 300 of nitrous gas. And if we add that the nitrous gas is composed of equal parts

parts of oxygen gas and azotic gas, as M. Gay Lussac had already demonstrated, we shall have a complete history of the combinations of oxygen and azote.

M. Guyton de Morveau, in a course of experiments on the diamond and the substances which contain carbon, has endeavoured to determine the action of the diamond on water at a very high temperature. The water was decomposed, and carbonic acid produced.

M. Sage has communicated to us his researches on the revivification of silver by mercury in the nitrate of silver; on an acetate of ammonia, extracted from wood by distillation; on the analysis of the calcareous stone, called the printing stone; on the magnesia contained in shells, madrepores, limestone and the arragonite; on an ore of *arenaceous* iron; on an unknown petrifaction; and on the analysis of a petrified wood which was both cupreous and ferruginous. We regret that our limits do not permit us to enter more into the details of these numerous inquiries.

When chemistry descends from crude to organized bodies, the phænomena which come under its observation are more complex, and the results obtained more obscure. Thus has this branch of chemistry been neglected until lately; and most of the observations and discoveries with which it is enriched are undoubtedly owing to the labours of M. Fourcroy, (whose loss we have now to deplore,) and to his distinguished friend M. Vauquelin.

This last chemist has lately been occupied with the analysis of tobacco, with a view to ascertain the principles which characterize this plant, and which have recommended it to general use; and, finally, with a view to appreciate the modifications which it undergoes, in order to prepare it for becoming an article of commerce. It results, from M. Vauquelin's inquiries, that the broad-leafed tobacco plant *(Nicotiana latifolia)* contains an animal matter of an albuminous nature, some malate of lime, with an excess of acid, acetic acid, nitrate and muriate of potash, a red matter the nature of which is unknown, muriate of ammoniac, and, lastly, an acrid and volatile principle, which seems to be different from all those known in the vegetable kingdom. It is this principle which gives to tobacco the qualities which we know it possess; we may separate it from the plant by distiltation, and employ it separately. Prepared tobacco presented more sensibly, than when unprepared, carbonate of ammonia and muriate of lime.

M. Vauquelin, thinking that the juice of bella donna, from having effects on the animal œconomy analogous to those

those of tobacco, contained the same acrid principle, proceeded to analyse it; but he found only an animal substance, salts with a base of potash, and a bitter substance from which the juice of the bella donna receives its narcotic properties.

Under the head *Physiology*, we shall allude to the experiments made by M. Vauquelin, on animals, with this juice.

M. Chevreul has presented to the class some very extensive experiments on vegetable substances. Some have for their objects, the bitter principle produced by the action of the nitric acid on organized bodies which contain azote, and which had already occupied the attention of Messrs. Haussman, Welther, Proust, Fourcroy, and Vauquelin.

M. Chevreul thinks that this bitter principle is composed of nitric acid and a vegetable oily or resinous substance; and he ascribes the property which this substance has of detonating, to the decomposition of nitric acid, to the formation of ammoniacal gas, of prussic acid, and oily hydrogen gas, &c. This agrees partly with the observations of Messrs. Fourcroy and Vauquelin.

But along with this bitter principle, a resinous matter and a volatile acid are produced, on which M. Chevreul has made various experiments, and which he regards as differing from the bitter principle only in consequence of its having a small portion of nitric acid.

A second inquiry of M. Chevreul has for its object the substances formed by the action of the nitric acid on charry or resinous bodies, and which have the property of precipitating gelatine. The first observations on this subject were made in England by Mr. Hatchett; and they induced a belief that these substances were analogous to tannin. M. Chevreul is of opinion that this is erroneous, and that they differ from each other, not only according to the kind of acid and other substance with which they have been prepared, but also according to the quantity of acid which has entered into their composition.

Lastly, pursuing always the same kind of experiments, M. Chevreul directed his attention to the different compounds formed by the reaction of the sulphuric acid on camphor. All the above researches obtained the approbation of the class, and were ordered to be inserted in the *Mémoires des Savans Etrangers*.

We ought to speak, perhaps, of the supposed discovery made by M. Vinterl, of an earth which he calls *andronia*, and in which he thinks he has found some extraordinary properties;

properties; and of the memoir of M. Pitaro, in which he endeavours to demonstrate, that a substance discovered in the Grotto de l'Arc, and analysed by M. Laugier, originates from the decomposition of the insects and reptiles which this grotto contains. But there are errors sometimes so palpable, that it is best perhaps to say nothing of them.

Every year has added to the store of knowledge we possess on the subject of the application of chemistry to the arts; and thus new proofs are afforded of the assistance which our wants, added to our industry, may derive from the sciences.

M. Chaptal, to whom manufacturers are already indebted for so many useful processes, has published some interesting observations on distillation from wines. We find, from the history which he gives of this art, by the description of the apparatus formerly employed, and that of the present day, that the processes for the production of spirits have been ameliorated in proportion to the improvements in chemical apparatus. One of the most important of these improvements, adopted in the South of France, consists of scarcely any thing else than the apparatus of Woulf on a large scale. The laws of evaporation, and the processes by means of which the liquids are heated by steam, have been ingeniously combined, in order to effect the distillation of spirits in an œconomical manner; but the observations of M. Chaptal will undoubtedly lead to new improvements in the preparation of spirits, and will contribute to preserve to this important branch of French trade, the superiority which it has acquired.

The same member has analysed seven specimens of colours found at Pompeia, which had been sent to him by the empress Josephine*.

M. Sage has been occupied in ascertaining the best processes for extracting quicklime, in order to obtain a solid mortar. He has also examined the nature of different pieces of stucco; the best method of giving the polish of marble to artificial stones; and, finally, he has given an account of a process for reducing white wax into soap.

The same author in a memoir, and Messrs. Guyton and Vauquelin in a report, have communicated some observations on the advantages and disadvantages of employing zinc in covering houses †. And on the request of the minister of the interior, the committee for chemistry has

* See Phil. Mag. vol. xxxiv. page 411.

† This is not a French discovery; nor is it new. See an ingenious paper by Mr. Randall on this subject in our 28th volume, p. 344.—Edit.

should

shown what are the descriptions of manufactories which are injurious to the health of the surrounding inhabitants; and measures have been suggested for removing such as are nuisances, without compromising the interests of the proprietors.

A report has been made to the Institute on a memoir by M. Tarry, relative to the composition of writing ink. The author has succeeded in making an ink which cannot be destroyed by the acids or alkalies, and which has only the slight inconvenience of allowing its colouring matter to be deposited rather too easily. "The discovery of M. Tarry," says the reporter, "promises a great benefit to society; viz. the introduction of an ink, which, not being susceptible of being obliterated by the chemical agents at present known, will put an end to the falsification of writings, which is but too common."

Another, on the artificial turquoises of M. de Sauviac, gives reason to hope that art will shortly rival nature in these productions, and furnish a new source of riches.

A committee has been busily employed in examining a process of the late M. Bachelier, for the composition of a preservative plaster of Paris. Houses built of stone are quickly covered with an earthy coating, of a dirty gray colour; and this first change is the cause of the deterioration which they soon afterwards undergo. A small kind of spider fixes his web in the hollows on the surface of the stone: these webs accumulate, and, with the dust which they collect, form the earthy crust just mentioned, in which lichens sometimes take root, and which naturally retain a constant humidity at the surface of the stones: the frosts then produce considerable injury, and give occasion for those raspings, which are in themselves a real deterioration.

A plaster therefore became a desideratum, which should fill up the inequalities of the stone without making the angles look clumsy, or deadening the carvings, and which should resist rain and other effects of weather. The late M. Bachelier had made some interesting experiments on this subject; and the above committee, aided by his son, have succeeded in producing a plaster which has resisted the tests to which they exposed it, and which gives fair grounds to expect that our buildings will in future be protected from the causes of decay above enumerated.

MINERALOGY.

Our labours in mineralogy will appear inconsiderable in comparison

comparison with those of which we have had occasion to give an account in former years.

M. Guyton has made us acquainted with a new crystalline form of the diamond. We know that the forms under which it is most frequently presented, are the regular octahedron and the dodecahedron with rhomboidal faces. The variety discovered by our associate is formed of two demi-spheroids, the returned position of which, imperfectly terminated at one of its extremities, presents at the other very clear re-entering angles, which characterize the form called hemitrope by M. Hany.

The same member, having directed his inquiries to the tenacity of metals, was led to some new experiments on the diminution of specific gravity in lead, in consequence of being put into a vice, as proved by Muschenbroek; and the cause of which remained unknown. Pieces of this metal were driven into ferules; and when the dies and the latter were adjusted, so that there could be no oozing out of metal, nor was the lead permitted to become soft, the lead in this case was found, like all the other metals, to increase in specific gravity by the operation.

M. Sage has communicated to the class his inquiries respecting emery, and the substances which are calculated to supply its place in polishing. It results from his observations, that the pulverized chrysolite of volcanoes may supply the place of emery. All the artists who employed it have been satisfied with its effects.

[To be continued.]

LII. *Intelligence and Miscellaneous Articles.*

DE LUC'S ELECTRIC COLUMN.

We learn, by a communication dated 23d April, that the small bells which were connected to De Luc's *electric column*, mentioned in our last number, ceased to ring for about a minute on the 24th March; and again on the same day for about three minutes. They were also supposed to have stopped for about half a minute the next day; but this is much doubted. Since that time they have been known once to cease ringing. On the 15th April, the closet where they are placed was opened, when the clapper was observed to vibrate with very great velocity. It is thought that the loudness of the sound is considerably increased of late; also that the vibrations of the clapper are quicker than when the apparatus was put into the closet on the 14th March. [April 28.]

The

The cranium of a horned animal, the race of which seems to be extinct, has been recently dug up at Oeltre, near Ninava, in Russia. From the description given of this part of the skeleton, the animal must have been at least 10 or 12 feet long; the horns which are attached to the head, and which have partly passed into a fossil state, far exceed in size those of the oxen of the present day: when measured at the root, they are a foot and a half in circumference, and two feet and a half long. After finding the cranium, several efforts were made to recover the entire skeleton, but two teeth only were found. Foreign naturalists are of opinion, that the head in question must have belonged to the race of *Urus* or *Aurochs*, mentioned by Cæsar in the 6th book of his Commentaries, and which are supposed to exist still in the mountains of Siberia, and even in the forests of Poland.

Sir George Mackenzie, accompanied by Mr. Henry Holland, and Mr. Richard Bright, of the university of Edinburgh, has sailed from Leith for Stromness; from whence they are to proceed to Iceland, in a vessel which is expected there from London.

The object of this arduous undertaking is to explore part of that inhospitable country, which, without British commiseration, would, in consequence of the war, be deprived of the absolute necessaries of life.

In the circumscribed state of our commerce, this country is very well worth the attention of Great Britain. In return for our coarse fabrics, we might procure such articles as Iceland, under proper management, would afford in great plenty; such as fish, oil, feathers, and sulphur, the scarcity of which last article has been such as to have already attracted the attention of parliament.

MEDICAL AND CHEMICAL LECTURES.

Dr. CLUTTERBUCK will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday, June the 4th, at a quarter before Ten in the morning, at his house, No. 1, Crescent, New Bridge Street; where further particulars may be had.

Mr. TAUNTON will commence his Summer Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, on Saturday, May 26th, 1810, at 8 o'clock in the evening precisely. The Lectures will be continued every succeeding Tuesday, Thursday, and Saturday, at the same hour, until the completion of the Course.

In the above Course of Lectures it is proposed to take a compre-

comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and treatment of surgical diseases, with the mode of performing the different surgical operations: forming a complete course of anatomical and physiological instruction, for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

Particulars may be had on applying to Mr. Taunton, Greville-Street, Hatton Garden.

LIST OF PATENTS FOR NEW INVENTIONS.

To Frederick Koenig, of Camden Town, in the county of Middlesex, printer, for a method of printing by means of machinery.—March 29, 1810.

To Jonathan Ridgway, of Manchester, plumber and glazier, for an improved method for preparing rollers and blocks used for calico printing.—April 6.

To John Stancliffe, of Caius College, Cambridge, bachelor of physic, for certain improvements in apparatus for combination and condensation of gases and vapours applicable to processes of distillation.—April 6.

To John Woodhouse, of Bromsgrove, in the county of Worcester, for several improvements relative to canals. —April 6.

To William Speer, late of the city of Dublin, but now of the city of Westminster, esq., for a new or improved method or process of increasing the inflammability and combustibility, and of improving the light of oils used for burning, particularly applicable to the oils refined according to the patent process, which will also improve oils refined according to the patent process, and oils when used for burning.—April 6.

To James Fussel, of Mells, near Frome, in the county of Somerset, iron manufacturer, for a method of making and working forge and other bellows.—April 6.

To Charles Frederick Davis, of the parish of Itchcombe, in the county of Gloucester, clothier, for an improvement in the manufacture of woollen cloth.—April 6.

To William Parr, of the Portland Hotel, Great Portland Street, in the county of Middlesex, esq., for his improved gunpowder.—April 11.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For April 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	44	52°	42°	29·59	0	Rain
28	44	50	40	·78	22	Fair
29	40	51	39	·90	20	Showery
30	40	49	42	·85	25	Cloudy
31	43	51	45	·62	0	Showery
April 1	45	49	42	·40	0	Showery
2	40	45	40	·72	15	Cloudy
3	43	56	46	·60	21	Cloudy
4	45	47	39	·30	0	Rain
5	37	47	45	·69	36	Fair
6	42	45	42	·22	0	Stormy
7	41	53	43	·30	31	Cloudy
8	44	51	43	·58	20	Cloudy
9	45	48	44	·45	0	Rain
10	44	42	39	·55	0	Rain
11	37	42	36	·65	5	Showery
12	33	41	34	·90	25	Cloudy
13	34	42	35	·89	20	Cloudy
14	36	47	39	·81	26	Cloudy
15	44	49	40	·71	36	Fair
16	39	47	40	·48	10	Showery
17	42	52	45	·64	20	Showery
18	48	56	46	·70	35	Cloudy
19	49	57	47	·80	42	Fair
20	48	57	45	30·10	39	Fair
21	49	59	47	·17	46	Fair
22	50	63	54	·16	40	Fair
23	54	66	55	·20	39	Fair
24	54	63	44	·15	40	Fair
25	44	58	43	·14	57	Fair
26	47	59	45	·12	65	Fair

N.B. The Barometer's height is taken at one o'clock.

LIII. *Description of an improved Apparatus for the Decomposition of Potash and Soda.* By William Johns, *Esq.*

To Mr. Tilloch.

Sir, The publicity of the Philosophical Magazine induces me to transmit for insertion the following account of some successful attempts to repeat the brilliant experiments of professor Davy, in the decomposition of potash and soda, on a more œconomical plan than has before been suggested*, and which I hope will enable your readers to obtain potassium, &c. for their use at a very moderate expense.

I am yours, &c.

William Johns.

April 16, 1810.
No. 3, Orford Row, Kent Road.

Early in the present year, wishing to decompose potass with the bent gun-barrel as is done at the Royal Institution, and not enjoying the ample resources of its excellent chemical professor; it became a desideratum to construct an apparatus, which might serve for the repetition of the experiment, instead of cutting the gun-barrel in pieces, as in the method before in use,—a practice attended with considerable expense, and which could not but check the ardour of those who are fond of chemical researches, and prevent the frequent repetition of the experiment: the disappointment resulting from an unsuccessful experiment being always much heightened when accompanied with the loss of an expensive apparatus.

Having succeeded in constructing an apparatus simple and efficient, and which has met the approbation of several eminent chemists, I herewith send you a drawing and description of it.

The apparatus consists of a common gun-barrel with one bend, and one of the ends inclining downwards a little. The inclining straight part is cut off from the bent portion at about three inches distance, is ground into it, being made to fit air-tight. Underneath is a small thin iron tube open at both ends, made a little conical, which when the apparatus is taken to pieces is placed within the interior of the straight piece, one half of it going into this part; the other will be received in the opposite part, when the apparatus is again put together. This small tube is to collect the potas-

* See Phil. Mag. vol. xxxii. p. 276.

sium in, and which purpose it answers exceedingly well, obviating an inconvenience and waste which sometimes happen from its being dispersed through the barrel.

In the first apparatus which I had constructed on this plan, I had the parts made to screw together: this answered extremely well, but it possesses no advantage over a ground joint, and is more expensive.

The first attempt to use this apparatus, I obtained less than 20 grains of the potassium: this was, however, encouragement, my apparatus being in every respect perfect as when I began the process; it served me twelve times very completely, and in the last experiment I collected 140 grains of the metalloid from eleven drachms troy of the alkali.

Having succeeded in preserving my apparatus (it at length was melted in a small place, the lute having fallen off in the process), my object was to substitute the common *caustic potass* instead of the pure potass which had been generally used. I tried it; and to my satisfaction obtained the result just mentioned. At this I was much pleased, the kali purum being sold at one eighth of the price of the pure potass. In my subsequent experiments twelve drachms yielded the extraordinary produce of 170 grains, the apparatus being taken out of the fire entirely free from being acted on.

Having attempted the caustic soda alone, and not succeeding with it, I used the proportions of two drachms of soda to six drachms of potass, and obtained 60 grains of a beautiful compound nearly fluid, of considerable lustre, and which lustre was inconsiderably diminished some days after; it nearly floated in the naphtha, being apparently of the same specific gravity.

From one part of soda to seven of potass, the proportion used by Mr. Davy in a similar experiment, I obtained from ten drachms 150 grains of metalloids, in appearance resembling quicksilver, equally fluid at a low temperature, though with this striking difference, of less specific gravity than some very pure naphtha, in which it floated.

The furnace I use is the common black-lead crucible, about eight inches diameter; I generally leave off the upper section as well as the flue. The furnace used at the Institution is, if I do not mistake, above 20 inches. The double bellows are small in proportion, about 36 inches by 14 inches.

In performing the experiment, it may be observed in addition to what has been said, I leave the stopper out of the

the potass tube till near the end of the process, bring it to a red heat, before any of the alkali is supplied; then the iron turnings being at a white heat, I introduce pieces of the potass in succession: these are immediately brought into igneous fusion, admitting of part of the water contained in it to pass off, and in this state it drops down on the iron. When the last portion is introduced, and this also is become red hot, the stopper is put into the tube and luted over. This is all done in less than ten minutes, from the first supplying of the potass tube with alkali: at the other end a glass tube passes down into some olive oil: here the hydrogen escapes, often in a state of brilliant combustion, and potassium deposits in the glass tube: this however should be prevented by wet cloths applied to the straight part of the gun-barrel.

Description of the Drawing (Plate IX.)

A. Potass tube.
B. Stopper to be put in at the end of the process.
C. The situation of the iron turnings.
D. The fire-place.
E. The grate.
F. The pipe of the bellows.
G. Straight part ground to the bent part.
H. Thin iron tube to be placed within the joint, to collect the potassium.
I. Glass tube.
K. Olive oil.
L. The part where the gun-barrel begins to decline downward. N. B. This inclination should commence precisely at the point where the tube emerges from the furnace, to prevent the potassium from flowing back on the iron turnings.
M. Stop-cock screwed into a socket fixed in the gun-barrel, which is shut the moment gas ceases to be given off, and which is known by the consequent absorption *.

LIV. *On the Composition and Decomposition of Forces. Translated from* "Traité élémentaire de Statique, par Gaspard Monge;" *by Mr.* W. Marrat, *of Boston, Lincolnshire**.

1. When a force P, (fig. 1 and 2,) applied to a determined point C, of the solid body AB, draws, or pushes

* Communicated by Mr. Marrat.

 this

this body in any direction CF; we may be permitted to consider this force as if it were applied to any other point D, *in the body*, which is in the direction of this force.

For as the several points of the body which are in the right line CF can neither approach towards nor recede from one another, it is evident that no point whatever in this line can move without moving all the others, in the same manner as if the force were immediately applied to them.

We may also be permitted to consider the force P as if it were applied to any other point G *out of the body*, in the direction of the force, provided that this point be invariably attached to the body.

2. It follows then, that if in the direction of the force P there be found either in the body a fixed point D, or out of it an immoveable obstacle G, provided that in this latter case the obstacle be invariably attached to the body, the force will be destroyed, and the body remain at rest; for we may regard the force as immediately applied to the fixed point, and its effect will be destroyed by the resistance of that point.

3. Reciprocally, if the force P applied to the body AB be destroyed by the resistance of a single fixed point, this point will be found in the direction of the force; for this point could not destroy the effect of the force unless it opposed the motion of the point of application C, and it could not prevent this motion unless it were in the right line in which the force tends to move the point of application.

AXIOMS.

I.

4. A point cannot move several ways at once.

5. Therefore, when several forces differently directed are applied at the same time to the same point, this point either remains at rest, or it moves in a single direction, in the same manner as if it were moved by a single force in that direction, capable of producing the same effect.

6. Thus, whatever may be the number and direction of forces applied at the same time to the same point, there always exists a single force which can move it, or which tends to move it, in the same manner as all the forces together. This single force is called the RESULTANT, and the several forces which compose the system, and act all together, are called COMPOSANTS. The operation by which we seek the *resultant* of several given *composant* forces, is

is called the COMPOSITION OF FORCES, and that by which we find the composants when we know the resultant, is called the DECOMPOSITION OF FORCES*.

II.

7. Two forces equal and directly opposite, applied at the same time to the same point, destroy each other and are in *equilibrio*. Reciprocally, when two forces are in equilibrio, they are equal and directly opposite.

8. If, therefore, several forces differently directed are applied to the same point, to produce an equilibrium, that is to destroy the effect of their resultant, we must apply to this point a single force equal to their resultant, and directly opposite to it; or else we must apply several forces the resultant of which may be equal and directly opposite to the resultant of the first.

9. Reciprocally, when several forces, differently directed, and applied to the same point, are in equilibrio, their resultant is nothing, or, which comes to the same, any one of these forces is equal and directly opposite to the resultant of all the others, or lastly the resultant of any number of forces is equal and directly opposite to the resultant of all the others.

III.

10. If several forces applied to the same point have all the same direction, and all act the same way, the effect produced upon this point is the same as would be produced by a single force equal to their sum, acting the same way, and in the same direction; consequently this single force is their *resultant*.

11. Hence, to produce an equilibrium with all these forces, we must apply to the same point, and in an opposite direction, a force equal to their sum; for this force will be equal and directly opposite to their resultant.

12. It follows then, FIRST, that when two unequal forces are applied to the same point, but in opposite directions, the resultant will be in the direction of the greater, and equal to their difference. For the greater of these two forces may be considered as composed of *two* forces, directed the same way, one of which is equal to the less and the other equal to their difference; now of these two latter forces, the first is destroyed by the less (7); therefore, there remains to move the point, only the difference, which acts in the same direction as the greater.

13. SECONDLY. That if ever so many forces be applied to the same point, some of which act one way and the

* English authors call this *resolution* of forces.

others the contrary; after having found the sum of all those which act one way, and also the sum of all those which act the contrary way, the resultant of all these forces is equal to the difference of these sums, and is directed the same way as the greater.

14. Therefore, to produce an equilibrium with all these forces, we must apply to the same point, and in the direction of the less sum, a force equal to the difference of the sums. For this force will be equal and directly opposite to their resultant.

THEOREM.

15. If to the extremities of an inflexible right line AB, (fig. 3) two equal forces P, Q, be applied, both of which act the same way, and the directions of which AP, BQ, are parallel to each other:

1. The direction of the resultant R of these forces is parallel to AP, BQ, and passes through the middle of AB.

2. The resultant is equal to the sum P+Q of these two forces.

Demonstration of the First Part. Let another inflexible right line DE perpendicular to the direction of the forces be invariably attached to the right line AB, and produce the direction of the forces P, Q, till they meet the right line DE in D and E; we may consider these forces as applied at D and E.

Divide DE into two equal parts in C, and on that side to which the forces tend to move this right line place an immoveable obstacle at C, and the right line ED will be at rest; for, the parts of the line on each side of the obstacle being equal, there is no reason why one of these equal forces should overcome the other; therefore the resultant will be destroyed by the obstacle C, or the resultant will pass through the point C. In the same manner it may be shown that the direction of the resultant of the two forces passes through I, the middle of any other right line GH parallel to DE.

Therefore, it passes at the same time through the two points C, I, which are equally distant from the direction of the forces P, Q, of course it is parallel to them, and passes also through the middle of AB.

Part II. The direction of the two forces P, Q, and that of their resultant R, being parallel, we may consider them as concurring in a point at an infinite distance, and the two forces P, Q, as both applied to this point: now the resultant of two forces applied to the same point is equal to their sum (10); therefore the resultant of the two forces, P, Q, is equal to the sum P+Q of those forces.

16. Cor.

16. *Cor.* 1. To produce an equilibrium with the forces P, Q, we have only to apply to the point K a third force equal to their sum, acting in a contrary direction, but parallel to AP or BQ: for this third force will be equal and directly opposite to their resultant.

17. *Cor.* 2. If after having divided an inflexible right line into any number of equal parts, we apply to all these points of division equal forces the directions of which are parallel among themselves; the resultant of all these forces passes through the middle of the right line in a direction parallel to that of the forces, and is equal to their sum.

For all the particular resultants of these forces taken two and two at equal distances from the middle of the right line, will pass through the middle, in the same direction, and each resultant will be equal to the two forces which are its composants: therefore, the general resultant will pass through the middle of the line (10), according to the same direction, and will be equal to the sum of all the particular resultants, or equal to the sum of all the composants.

THEOREM.

18. If to the extremities of an inflexible right line AB (fig. 4.) two unequal forces P, Q, be applied, the directions of which AP, BQ, are parallel and act the same way:

1. The resultant R is equal to their sum, and its direction is parallel to that of the forces.

2. The point of application C, of the resultant R, divides the right line AB into two parts which are reciprocally proportional to the forces; or in such a manner that

$$P : Q :: BC : AC.$$

Demonstration of First Part. For divide the right line AB into two parts in D, directly proportional to the forces P, Q, and we shall have P : Q :: AD : DB.

Produce the inflexible line AB both ways, and make AE = AD and BF = BD, also conceive that the two forces P and Q are uniformly distributed through every point of the right line EF, and act in directions parallel to the direction of the two forces P, Q; it is evident that the force P will be distributed through the part DE, and that the force Q will be distributed through the part DF.

Moreover, the force P will be the resultant of all the forces distributed through ED, and Q that of all the forces distributed through the line DF (17): hence the general resultant of all the forces uniformly distributed through the line EF, will pass through C, the middle of EF, and according to a direction parallel to the direction of the

 composants,

composants, and is equal to their sum: therefore, the resultant of the forces P, Q, is equal to their sum, and its direction, which passes through C, is parallel to BQ or AP.

2. The right line AB being the half of EF, we have AB=EC, and by subtracting from each of these equal quantities the part AC, which is common, we have BC=EA=AD.

Also, because AB=CF, by taking away the common part CB, we have AC=BF=BD, and because, by supposition, P : Q : : AD : DB,
we shall have ..... P : Q : : BC : AC.

But the resultant of the two forces P, Q, has been proved to pass through C: hence the point of application of this resultant divides the right line AB into two parts which are reciprocally proportional to the two forces.

19. *Cor.* 1. Therefore, to produce an equilibrium with the two forces P, Q, we must divide the right line AB in C, so that the two parts may be reciprocally proportional to these two forces, and apply to the point C a third force equal to the sum P+Q, in a contrary direction but parallel to AP or BQ.

20. *Remark.* If the relation of the forces P, Q, and the length of the right line AB be given in numbers, and we want to determine the distance of the point C from A or B, the proportion

P : Q : : BC : AC

cannot be immediately applied, because in this proportion we only know the two first terms: but since

P : Q : : BC : AC,
by composition P+Q : Q : : BC+AC=AB : AC;
also P+Q : P : : AB : BC;

in each of which the three first terms are given.

21. *Cor.* 2. When a single force R is applied to a point C, in the inflexible right line AB, we can always resolve it into two others P, Q, which being applied to the two given points A, B, and directed parallel to RC, will produce the same effect; for the force R may be divided into two parts which are reciprocally proportional to the line AC, BC, by means of the two following proportions

AB : BC : : R : P
AB : AC : : R : Q.

In each of which we know the three first terms. And the resultant of the two forces P, Q, has the same quantity and direction, and acts the same way as the force R.

22. *Cor.* 3. Every thing being (in fig. 5) as in the preceding corollary, if we apply to the point C, of the right line

line AB, a force S equal and directly opposite to the resultant of the two forces P, Q, in such a manner that $S=R=P+Q$; the three forces P, Q, S, will be in equilibrio (19), and either of the two forces P, Q, may be regarded as equal and directly opposite to the resultant of the other two. Hence the resultant of two forces S, Q, of which the directions are parallel, and which act contrary ways, is a force *p*, equal and directly opposite to the force P. Now the force P is equal to the difference of the forces S, Q, and acts in a direction contrary to that of the greater S of these two forces; therefore, 1. The resultant *p* of the two forces S, Q, is equal to their difference $S-Q$, and it acts the same way as the greater, in a direction parallel to that of these two forces.

Moreover we have $P+Q$ or $S : Q :: AB : AC$ (20).

2. The distances of the point A of application of this resultant from the two points C, B, are reciprocally proportional to the forces S, Q.

23. *Remark.* If the ratio of the two forces S, Q, and the length of the right line BC are given in numbers, and we would find the distances of the point A from the points B, C, the preceding proportion cannot be directly applied, because in this proportion we know only the two first terms; but from it we easily deduce this by division of ratios; viz. $S-Q : Q :: AB-AC$ or $BC : AC$ in which we know the three first terms.

We find the distance AB by this other proportion,

$$S-Q : S :: AB-AC \text{ or } BC : AB.$$

24. *Cor.* 4. If the two forces S, Q, the directions of which are parallel and which act contrary ways, are equal between themselves, 1. their resultant P, which is equal to $S-Q$, (22) becomes nothing; 2. in the proportion $S-Q : Q :: BC : AC$, the second term being indefinitely great with respect to the first, which is nothing, the fourth term AC is also indefinitely great with respect to the third. Therefore, the point A of application of the resultant P is at an indefinite distance from the point C. Hence, to produce an equilibrium with the forces S, Q, we must apply to the inflexible right line a force equal nothing, the direction of which passes at an indefinite distance: this is not absurd, but it cannot be executed.

We see then that it is impossible, by means of a single force, to produce an equilibrium with two equal forces the directions of which are parallel and which act contrary ways; but by means of two forces we can produce with them an equilibrium in an indefinite number of ways.

PROBLEM

PROBLEM.

25. Any number of forces P, Q, R, S, &c. the directions of which are parallel, and which act the same way, being applied to the points A, B, C, D, &c. given in position, (fig. 6) and connected in an invariable manner; to determine the resultant of all these forces.

SOLUTION. Take any two of the forces, as P, Q, and determine (20) their resultant T; this resultant will be equal to $P+Q$, and its direction will be parallel to that of the two forces P, Q, and we find its point of application E by the following proportion, $P+Q : Q :: AB : AE$.

Instead now of the two forces P and Q we may substitute their resultant T, and having drawn the right line EC, we must determine the resultant V of the two forces T, R; this resultant V will also be that of the three forces P, Q, R; its quantity equal to $T+R$ or $=P+Q+R$, and its point of application F will be found by this proportion,

$$T+R \text{ or } P+Q+R : R :: EC : EF.$$

Instead of the three forces P, Q, R, we may now substitute their resultant V, and after having drawn the right line FD, we must find the resultant X of the two forces V, S; this resultant X will be that of the four forces P, Q, R, S, and its quantity equal $P+Q+R+S$; and we determine upon FD its point of application G, by the proportion, $V+S$ or $P+Q+R+S : S :: FD : FG$.

In the same manner we may proceed for any number of forces whatever, and the quantity of the last resultant will be equal to the sum of all the forces in the system.

26. *Cor.* 1. Hence, by supposing that the point G is invariably connected to the points A, B, C, D . . . we shall have an equilibrium with all the forces P, Q, R, S . . . by applying to the point G a force, the direction of which is parallel to that of the original forces, which acts the contrary way, and which is equal to their sum $P+Q+R+S$ . . . .

27. *Cor.* 2. If among the forces P, Q, R, S, . . . the directions of which are parallel, some of them act one way and the remainder the contrary way; we first determine the particular resultant of those which act one way, and then the particular resultant of all those which act the contrary way. For all the forces being reduced to two acting in opposite directions, in determining by art. 23 the resultant of these two forces we have the general resultant, and, consequently, the force which, if applied in a contrary direction, would keep the whole in equilibrio. The general resultant being equal to the difference between the two

two particular resultants (22), and each of these being equal to the sum of those which compose it (25), it follows that the general resultant is equal to the excess of the sum of the forces which act one way, above the sum of the forces which act the contrary way.

28. *Cor.* 3. If the forces P, Q, R, S, ...remain parallel among themselves, and without changing in quantity, take another direction, and become $p, q, r, s, \ldots.$, the resultant t of the two first will still pass through E and be equal to their sum $p+q$. Likewise the resultant v of the three forces p, q, r passes through the point F, and is equal to $p+q+r$. In the same manner the resultant x of the four forces p, q, r, s will pass through the point G and will be equal to the sum $p+q+r+s$. Hence the general resultant of all the forces p, q, r, s.. will always pass through the same point as the general resultant of the first forces P, Q, R, S, &c. . . .

We see then that when the quantities and the points of application of parallel forces remain the same, the resultant of these forces always passes through the same point whatever may be their direction, and the quantity of this resultant is always equal to their sum.

The point through which the resultant of parallel forces always passes, whatever may be their direction, is called *the centre of parallel forces.*

It is easy to perceive that if the points of application A, B, C, D, . . . of the parallel forces P, Q, R, S . . . .are in the same plane, the centre of these forces is in the same plane; for this plane contains the right line AB, and consequently the point E in this right line, which is the centre of the forces P, Q: it contains also the right line EC, and of course it contains the centre F of the forces P, Q, R: it also contains the right line FD, and consequently it contains the centre G of the forces P, Q, R, S; and so on.

If the points of application are in the same right line, we can demonstrate in the same manner that the centre of parallel forces is in the same right line.

THEOREM.

29. Two forces applied to the same body cannot have a resultant, unless their directions concur in the same point, and are contained in the same plane.

Demonstration. When the directions of two forces do not concur in the same point, they cannot be considered as destined to move a single point; therefore a single force cannot produce the same effect, and consequently they have no resultant.

THEOREM.

THEOREM.

30. If the directions of two forces P, Q, applied to two points A, B, (fig. 7 and 8) of the same body, are contained in the same plane and concur in a certain point D:

1stly. The direction of the resultant of these forces will pass through the point of concourse D;

2dly. The direction of the resultant is in the same plane as the two forces P, Q, which are its composants.

DEMONSTRATION OF 1. The point D being found in the direction of both the forces, if we suppose that this point is connected to the body in an invariable manner, we may consider the two forces P, Q, instead of being applied to the points A, B, as applied to the point D, and that they have no other effect than a tendency to move this point; therefore, their resultant may also be considered as having no other effect. Now a single force cannot act upon a single point unless it be immediately applied to this point. Therefore the resultant of the two forces P, Q, may be regarded as applied to this point. Hence, the direction of this force passes through the point of concourse of its two composants.

DEMONSTRATION OF THE 2D. If at the two points of application A, B, we conceive an inflexible right line to be attached, in an invariable manner, the effect of the two forces P, Q, and consequently that of their resultant, is evidently a tendency to move the right line AB. Now a single force cannot move a right line unless it be immediately applied to some point in this line. Therefore, the resultant of the two forces P, Q, may be considered as applied to some point in the right line AB. Hence the direction of this force passes at the same time through the point D, and also through a point in the right line AB; it is therefore comprised in the plane of the triangle ABD, determined by the directions of the two composants P, Q.

31. *Cor.* It follows then that if three forces P, Q, R, applied to the same body, are in equilibrio among themselves, the directions of these three forces concur in the same point D, and are comprised in the same plane.

For these three forces being in equilibrio, any one of them is equal and directly opposite to the resultant of the other two; consequently any two of these forces have a resultant; therefore, (29) the directions of these two forces are comprised in the same plane, and concur in the same point; and the direction (30) of the resultant of these two forces, and consequently that of the third force which

keeps

keeps them in equilibrio, passes through the point of concourse, and is comprised in the plane determined by their directions.

LEMMA.

32. If a power P be applied to the circumference of a circle moveable about its centre A (fig. 9), according to the direction BP, which is a tangent to the circle at the point B; this power has the same tendency to turn the circle about its centre, as if it were applied to any other point C, and in the direction CQ, which is also a tangent at the point C.

THEOREM.

33. When the directions of two forces P, Q, are in the same plane, and concur in the same point A (fig. 10), if we take upon these directions the right lines AB, AC, proportional to these forces, in such a manner that

$$P : Q :: AB : AC;$$

and having completed the parallelogram AB DC; the direction of the resultant of these two forces will be according to the diagonal AD of the parallelogram.

Demonstration. Conceive for a moment that the point D is an immoveable obstacle, and from this point upon the directions of the two forces let fall the perpendiculars DE, DF; the triangles BED, CFD are similar, because the angles at B and C being each of them equal to the angle A, are equal to each other; therefore we have

$$DC : DB :: DF : DE;$$

& by the supposition $P : Q :: AB : AC$ or $:: DC : DB$;

therefore, $P : Q :: DF : DE$.

From the point D as a centre, with the radius DF, describe the circular arc FG, meeting ED produced in G; then, regarding this arc and the right line EG as inflexible lines invariably connected to the point A; conceive that the force P is applied at E, in the direction EP, and that a force M, equal to the force Q, is applied to the point G, in a direction parallel to AP, and consequently in the direction of a tangent to the arc FG; and because the force $M=Q$, and $DF=DG$, we have

$$P : M :: DG : DE.$$

Therefore (18) the resultant of the two parallel forces P, M, passes through the fixed point D, and is destroyed by the resistance of this point; also these two forces are in equilibrio about this point.

Now the force Q, the direction of which is a tangent to the arc FG, and which we may regard as being applied to the

the point F of its direction, tends to turn this arc in the same manner as the force M (32), and may be substituted for it in order to counterbalance the force P: therefore the two forces P, Q, are also in equilibrio about the fixed point D, and their resultant will be destroyed by the resistance of this point, and consequently the direction of this resultant will pass through the point D.

But we have seen that the resultant of the two forces P, Q, passes through the point of concourse A of their directions (30); therefore this resultant will be directed according to the diagonal AD.

34. *Cor.* 1. If from any point D taken in the direction AD of the resultant of two forces P, Q, we draw the right lines DB, DC, parallel to the directions of these forces, we shall have a parallelogram ABCD, the sides of which AB, AC, are proportional to the forces P, Q; that is, we shall have P : Q : : AB : AC or : : DC : DB.

For if these sides are not proportional to the forces, their resultant will be directed according to the diagonal of the parallelogram, the sides of which *are* proportional to these forces (33), and not according to AD; which is contrary to the supposition.

35. *Cor.* 2. If from the point D, taken upon the direction AD of the resultant of two forces P, Q, we let fall the perpendiculars DE, DF, upon the directions of these forces; these perpendiculars will be reciprocally proportional to the forces P, Q.

For it has been shown above (34) that P : Q : : DC : DB; and the triangles DBE, DCF, being similar, give

DC : DB : : DF : DE;

therefore P : Q : : DF : DE.

THEOREM.

36. When the directions of two forces P, Q, are comprised in the same plane, and concur in a point A (fig. 11); if we take upon these directions the right lines AB, AC, proportional to these forces, in such a manner that we have

P : Q : : AB : AC;

and having finished the parallelogram AB DC; the resultant R of these two forces will be represented in quantity and direction by the diagonal AD of the parallelogram; that is, we shall have

P : Q : R : : AB : AC : AD.

Demonstration. We have already seen (33) that the resultant of the two forces P, Q, will be directed according to the diagonal AD of the parallelogram; it therefore remains

mains only to show that its quantity will be represented by this diagonal. To the point A apply a force S equal and directly opposite to the resultant R, this force will be directed according to the prolongation of the diagonal DA, and the three forces P, Q, S, will be in equilibrio. Therefore the force Q, will also be equal and directly opposite to the resultant of the two other forces P, S; and consequently this last resultant will be directed according to the prolongation of the right line CA. Produce CA till AH =AC, and draw the right line HB, which will be parallel to AD, and consequently to the direction of the force S, also from H draw HK parallel to the direction of the force P; the two forces P, S, are proportional to the sides AK, AB, of the parallelogram ABHK, (34); that is, we shall have P : S : : AB : AK or HB.

Now because ADBH is a parallelogram, we have HB= AD; moreover the forces S and R are equal, therefore

P : R : : AB : AD.

But by supposition P : Q : : AB : AC;
therefore, by uniting these two proportions, we get

P : Q : R : : AB : AC : AD.

37. *Cor.* 1. If the two forces P, Q, are applied to the point A, they will be in equilibrio with a third force applied to the same point, the direction of which is DA, and which is proportional to the diagonal AD; for this force will be equal and directly opposite to the resultant of the forces P, Q.

If the forces P, Q, are applied to any other points of their directions, there will be an equilibrium in applying to any point whatever of the right line AD, and in the direction DA, a force proportional to AD; provided that the point of application of this last force be invariably connected to the points of application of the forces P, Q.

38. *Cor.* 2. We can always decompose a force R, given in quantity and direction, into two other forces P, Q, directed according to the given right lines AP, AQ; provided that these directions and that of the force R, are comprised in the same plane, and concur in the same point A.

For let the force R be represented by a part AD of its direction, and drawing, through the point D, the right lines DC, DB, parallel to the given directions AP, AQ, they form the parallelogram ABDC, the sides of which AB, AC, represent the forces P, Q, required; for (36) the resultant of these forces will have the same quantity and direction

rection as the force R. We find the quantities of the forces P, Q, by means of these proportions,

$$AD : AB :: R : P$$
$$AD : AC :: R : Q.$$

PROBLEM.

39. To determine the *resultant* of any number of forces P, Q, R, S, &c. whose directions, comprised or not comprised in the same plane, concur in the same point A.

DEMONSTRATION. Upon the given directions lay off, from the point A, the lines AB, AC, AD, AE.... proportional to the forces P, Q, R, S.... respectively; and considering, at first, any two of these forces as P, Q, and completing the parallelogram ABFC, the diagonal AF will represent the quantity and direction of the particular resultant T of these forces (36).

Instead of the forces P and Q we may now take their resultant T, and considering the two forces T, R, represented by AF and AD, complete the parallelogram AFGD, and the diagonal AG will represent the quantity and direction of the resultant V of the forces T, R, or of the three forces P, Q, R. Again, instead of the three forces P, Q, R, we may take their resultant V, and considering the two forces V, S, complete the parallelogram AGHE, the diagonal of which AH will represent the quantity and direction of the resultant X of the two forces V, S, or of the four P, Q, R, S.

In the same manner we may proceed to determine the resultant of any number of forces whatever, and the last resultant will be the general resultant of the system.

40. *Cor.* If all the forces P, Q, R, S.... are applied to the point of concourse A of their directions, to produce an equilibrium, we must first find the quantity and direction of their resultant, and then apply to the point A a force equal and directly opposite to it. But if the forces are applied to other points of their directions, invariably connected among themselves; to produce an equilibrium we must apply to any point of the direction of their resultant a force which shall be equal and directly opposite to that of their resultant, provided that the point of application of this force be also connected in an invariable manner to those of the forces P, Q, R, S....

PROBLEM.

41. To determine the resultant of any number of forces P, Q, R, S

P, Q, R, S, &c. whose directions comprised in the same plane do not concur in the same point; whose points of application A, B, C, D, &c. are connected together in an invariable manner; and whose magnitudes are represented by the parts A*a*, B*b*, C*c*, D*d*, of their directions.

Solution. After having prolonged the directions of any two of these forces, such as P, Q, until they have met somewhere in a point E; we lay off from E to F and from E to G the lines A*a*, B*b*, representing these forces, which will complete the parallelogram EF*e*G, whose diagonal E*e* will represent in magnitude and direction the resultant T of the two forces P, Q, (36).

Instead of the forces P, Q, we take the resultant T, and prolong its direction as well as that of the force R until they meet somewhere in a point H; we lay off the line E*e* from H to I, and the line C*c* from H to K; which will complete the parallelogram HI*h*K, whose diagonal H*h* will represent in magnitude and direction the resultant V of the two forces T, R, which will also be that of the three forces P, Q, R.

In the same manner, instead of the three forces P, Q, R, we take their resultant V, and prolong its direction as well as that of the force S until they meet in a point L; then laying off from L to M and from L to N the lines H*h*, D*d*, which represent the forces V and S, they complete the parallelogram LM*l*N, whose diagonal L*l* will represent the resultant X of these two forces, which is also that of the four forces P, Q, R, S.

By proceeding thus we may find the magnitude and direction of the general resultant of all the proposed forces, whatever may be their number.

42. *Cor.* Therefore when several forces, directed in the same plane, are applied to points connected together in an invariable manner, these forces have always a resultant; so that it is possible to make them in equilibrium by means of one force only; except in the case where the direction of one of these forces being parallel to that of the resultant of all the others, this force and this resultant are equal to each other, and act in contrary directions: for we have seen (24) that then to make them in equilibrium it is necessary to apply a force of nothing, whose direction should pass to an infinite distance; which is impracticable.

THEOREM.

43. If three forces P, Q, R, have their magnitudes and directions represented by the three edges AB, AC, AD, contiguous

tiguous to the same angle of a parallelopipedon ABFEGD, in such a way that P : Q : R : : AB : AC : AD, their resultant S will be represented in magnitude and direction by the diagonal AE of the parallelopipedon contiguous to that angle, and we shall have P : Q : R : S : : AB : AC : AD : AE.

DEMONSTRATION. On the face ABFC, which contains the directions of the two forces P, Q, let the diagonal AF be drawn; let the diagonal DE also be drawn on the opposite face DHEG; these two diagonals are parallel and equal, for the two edges AD, EF, of the parallelopipedon, at the extremities of which they terminate, are parallel and equal; therefore AFED will be a parallelogram. Hence the two forces P, Q, being represented in magnitude and direction by the sides AB, AC, of the face ABFG, which is a parallelogram, their resultant T will be represented in magnitude and direction by the diagonal AF, and we shall have P : Q : T : : AB : AC : AF.

In like manner the two forces T, R, being represented by the sides AF, AD, of the parallelogram AFED, their resultant S, which is also that of the three forces P, Q, R, will be represented by the diagonal AE of the same parallelogram, and we have T : R : S : : AF : AD : AE; therefore, by uniting the above two series of proportionals we have P : Q : R : S : : AB : AC : AD : AE.

Now the diagonal AF is likewise that of the parallelopipedon; therefore the resultant of three forces will be represented in magnitude and direction by the diagonal of the parallelopipedon.

44. *Cor.* We may always decompose a force S given in magnitude and direction into three other forces P, Q, R, directed according to the three given lines AP, AQ, AR, not comprised in the same plane, provided these three directions and that of the force S concur in the same point A.

To effect this, by the three directions considered two and two we draw the three planes BAC, CAD, DAB; he force S will be represented by a part AE of its direction; and by the point E we draw three other planes, EGDH, EHBF, EFCG, respectively parallel to the three first; these six planes are the faces of a parallelopipedon, whose diagonal is AE, and whose edges AB, AC, AD, which are taken on the three given directions, represent the magnitude of the forces required, P, Q, R; for (43) the resultant of these three forces will have the same magnitude and direction as the force S.

Otherwise, we draw through the point E three right lines

lines parallel to the directions AP, AQ, AR; and the parts EF, EH, EG, of these right lines, comprised between the point E and the planes BAC, CAD, DAB, represent the magnitudes of the required forces P, Q, R; for these lines being three edges of a parallelopipedon, they are respectively equal to the other edges AB, AC, AD, which are parallel to them.

LV. *Description of the French Telegraphs used on the Coasts of Flanders, &c. with Observations on the same; and a Plan of a Polygrammatic Telegraph on a new Construction. By Capt.* C. W. Pasley, *of the Royal Engineers**.

Having last year had an opportunity of observing the nature of the French telegraphs used on the coast of Flanders, of which no account has, to my knowledge, been published in this country, a description of them may perhaps be acceptable to the public; particularly as I am led to believe that they have been very recently invented, or at least established, and that they are now in general use throughout the whole extent of coast of the French empire. Various naval officers, whom I have consulted with a view of obtaining information on the subject, agree, that it is only a few years since these telegraphs first made their appearance; but I have not been able to ascertain the precise period. It does not seem, however, from what I have learned, to be more than three years ago, since the system of signals by masts, yards and balls, which I know was formerly used on many parts of the enemy's coast, was abolished, in order to make room for this new establishment.

Every telegraph consists of one upright post, to which are attached three arms exactly similar to each other, moving each upon its own distinct spindle or axis. The axis of one of these arms is near the head of the post. The distance from the centre of motion of each of the two uppermost arms to that of the one immediately below it, is rather less than double the length of one arm. The highest of the three arms (A) can exhibit seven distinct positions, but the other two arms (B and C) can only exhibit six positions each. The total number of combinations, or of distinct signals, which can be made by this

* Communicated by Capt. Pasley.

 telegraph,

telegraph, will consequently be three hundred and ninety-one*. As only three bodies are employed in the French telegraph, it may therefore appear superior to the Admiralty telegraphs used in England, which by the combination of double that number of bodies can only make sixty-three distinct signals.

The mechanism of the French telegraphs just described, must either be imperfect, or the men employed in working them must have been very unskilful, for the signals were made and repeated in an awkward manner, with, what seemed to me, much unnecessary loss of time; but these defects, it will be evident, detract nothing from its merit as an invention. In regard to the mechanical construction, I could only observe that the arms, which are painted black, and appear solid at a distance, are made in the fashion of a Venetian blind, in order, it may be presumed, to diminish the action of the wind in bad weather. Each arm has a counterpoise of thin materials painted white, which, unless the observer is very near the telegraph, becomes invisible.

In the annexed Plate, fig. 1 shows the telegraph in a state of rest, the dotted lines marking the several positions in which the arms can be exhibited.

Figs. 2 and 3 are a specimen of the telegraph at work.

Fig. 4 shows the construction of one of the arms on a larger scale, DE being the part which is fashioned like a Venetian blind, and EF the counterpoise.

From the above description it will appear, that the French have adopted, in the arms of their new telegraph, the same principle of motion used in the polygrammatic telegraph invented by me, of which an account is contained in the xxixth volume of this work; the only difference being, that in my telegraph two arms are placed on the same axis, instead of one, to which they have confined themselves.

Instead of using several posts, which appeared to me the most eligible mode, a polygrammatic telegraph may also be constructed upon one post. Fig. 5 shows how this may be done, on comparison of which with my former invention, (fig. 6,) it will be seen that the same signals can in some cases be made by both.

The disadvantage attending a polygrammatic telegraph

* A naval officer who has been lately employed in the Mediterranean informs me, that he does not believe that the arm A is ever shown in its fourth position. I think I have seen it myself in that position, but am not certain. If the French confine themselves to only six positions upon that arm, the total number of signals will be reduced from 391 to 342.

constructed

constructed upon one post instead of several, is, that in the former the combinations are more limited in number. The signal made at A in fig. 5, for instance, cannot be made either at B, C, or D in the same figure, although it may be made on all the posts A, B, C and D in fig. 6. Hence, whilst each post in fig. 6 can make twenty-eight distinct signals, that number in fig. 5 is limited to the part A alone, the signals that may be made at B, C or D, in fig. 5, being only twenty-one.

A polygrammatic telegraph upon one post, on the principle shown in fig. 5, may be thought, however, sufficiently powerful and copious.

If four pair of arms are used, the total number of distinct signals that may be made by the parts A, B, C and D combined, will be no less than 308,791

If only three pair of arms are used, in which case the upper part of the telegraph may be supposed cut off at the letter B in fig. 5, the number of distinct signals that may be made by this kind of telegraph (by the various combinations upon the parts B, C and D) will be 13,935.

If we suppose all the upper part of the telegraph to be cut off a little above the letter C in fig. 5, so that only two pair of arms are used, the number of distinct signals that may be made by this telegraph (by the combinations upon the parts C and D alone) will be 637.

Hence, even in this reduced state, my polygrammatic telegraph, whether constructed upon one or upon two posts, will preserve a considerable superiority over the French as well as over the British Admiralty telegraphs; and it may perhaps be allowed, that, in all cases, it will be perfectly clear, and as little liable to mistake, as any other telegraph that has been invented.

A disadvantage attends the polygrammatic telegraph upon four posts, in the form in which it was originally published (fig. 6), arising from the great space upon which the posts must stand; so that, if the arms are supposed to be six feet long, (measuring from their centre of motion,) the distance AD in that figure can hardly be less than forty feet. This disadvantage will be done away by making the posts of unequal heights, in the manner shown in fig. 7. By this method, the arms being still supposed six feet long, the distance AD may be reduced to twenty-two feet; so that the telegraph may be conveniently fitted to the roof of the smallest building.

C. W. Pasley,
Captain Royal Engineers.

 LVI. *On*

LVI. *On Crystallography. By M.* Haüy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 277.]

OF THE CHARACTERS OF MINERALS.

We understand by the term *characters* of a mineral, every thing that can be the subject of an observation proper for making it known. We could not refrain, in treating of the mineralogical methods in the preceding article, from giving an idea of the characters which are the soul of the system. But it is necessary to enter into more extensive details on this important subject.

If we consider the characters relative to the various branches of science which furnish them, we must distinguish them as physical, geometrical, and chemical characters.

The physical characters are those the observation of which produces no remarkable change in the state of the substance which presents them; or with respect to which this change is only a condition necessary for observing an effect which in other respects belongs to physics. Thus the phosphorescence produced by throwing the dust of a mineral upon burning coals, although it occasions an alteration in the state of the mineral, will be a physical character, like that which arises from the mutual friction of two pieces of quartz. In cases of this kind, where physics and chemistry are so closely allied that it would be difficult to discern their respective limits, we have had it particularly in view to preserve the analogy of the characters, by bringing together those which give rise to observations of a similar nature.

Properly speaking, we ought to denominate as geometrical characters those only which are drawn from the determination of the primitive forms, and from the measurement of the angles which form by their meeting the faces of the crystals and the sides of these same faces. But we have thought it right to give to this character a greater extent than that which seems to agree with it when we take it in a rigorous sense, and to include within it every thing which has a reference to the configuration; such as the aspect of the fracture, which sometimes forms convexities and concavities, and sometimes presents points or asperities, &c. Besides, we consider, independently of this aspect, the direction in which the fracture takes place, which is sometimes longitudinal, or parallel to the axis of the

the crystals, sometimes transversal, or perpendicular to the same axis, which also tends to refer it to the geometrical characters. Perhaps it may be said that it would have been more agreeable to change the word *geometrical* into another, which would have indicated in a looser manner the modifications depending on the configuration. But this word is so well adapted to those of *physical character* and *chemical character*, that we have preferred preserving it, by explaining the signification which it ought to have in the language of mineralogy.

The chemical characters are those which are proved by the decomposition of a mineral, or a sensible alteration in its nature, or a rupture of aggregation between its molecules. Such are the characters which are drawn from the action of the acids, from fusion with or without addition, by the intermedium of the blowpipe, &c.

It is from the assemblage of these three orders of characters that the character will be formed which we call *specific*, or that which will serve to distinguish all the bodies comprehended within one and the same species. We ought not to be afraid, from the reasons which we have already given, of multiplying the particular indications of which it is the assemblage, in order to procure the facility of making them serve for mutually verifying each other, or even of substituting the one for the other.

But is it not also an inconvenience, that the table of the characters of a mineral is so overloaded, that we are obliged to run over the whole of it without fixing on any thing which can give a precise knowledge of this mineral, and assist the mind in representing it as it were in miniature? It is with a view to obviate this inconvenience that I have adopted a character which I call *essential*, and which is composed of the smallest possible number of particular characters taken from among those of the species which are proper for distinguishing the latter from all others. Thus the essential of the telesia consists in its having a specific gravity of about 4, and presenting joints only in a direction perpendicular to the axis of the crystals;—that of the chabasy consists in its dividing into a rhomboid a little obtuse, and it melts easily in the blowpipe:—that of borated magnesia consists in the crystals of this mineral being electrical by heat in eight points opposed to each other in pairs;—that of sulphurated molybdenum is to leave metallic traces on paper, and to communicate electricity to resin by friction. The characters which compose what I call *essential*, will not be observable in all cases: but it will be always

correct to say that they belong exclusively to a certain species of mineral, in such a manner that the idea which they originate will be the faithful representation.

The character in question will be placed at the head of those which compose the specific character. It would, perhaps, be pushing the matter too far, to require that it should always distinguish precisely the substance to which it is applied, not only from all those of the same class, but in general from all the minerals. It would seem to be allowable to understand the classical name from its representative (*enoncé*), in such a way as to form with this name the entire definition of the substance for which it has been chosen. In this manner we define telesia, an earthy substance with a specific gravity of about 4, and which only presents well defined joints perpendicularly to the axis of its crystals. We shall see, however, that in a great number of cases the essential character taken by itself gives an exclusion to all the minerals different from that which it designates.

To conclude:—I do not flatter myself with having always succeeded in making the best possible choice of the characters which ought to form that which I call *essential*; and they will be found sometimes a little vague, when they refer to substances of which we have as yet but a slight notion. Time will add to our stock of knowledge, and this will serve to give more edge to those parts of the picture which are too feebly marked in the present state of science.

Still, however, this was not enough; and one of two things may have happened. Either the observer, who wished to determine a mineral, would proceed straight forward to the species of which this mineral formed a part, and then he would have nothing else to do but to consult the essential and specific characters, to ascertain that he was right; or, deceived by a false resemblance, he would be led to a foreign species. To bring him back to the right path in this last case, we have added, at the end of the specific character, another which we call *distinctive*, composed of the principal differences which may enable us to pick out a mineral from among those with which we should be tempted to confound it.

We have also placed at the head of each class a general view of the substances which it contains, with the enumeration of the characters, the assemblage of which may serve to distinguish this class from the others; and we have endeavoured to restrict these characters, so that there may

not

not result a picture too much overcharged. Our method being founded on analysis, it is only as it were by accident that certain divisions suit an assortment of characters which most frequently vary in a respect quite different from that to which the combination of the component principles is subjected. After all, if we examine the systems in which arbitrary terms most prevail, those in which the characters themselves have led the way for the distribution of bodies instead of following it, we shall perceive that it will often happen that general divisions are therein clearly circumscribed. Almost continually we find substances emerging from the limits within which they were supposed to have been confined. The important point is, that the species are well determined; because, as we have remarked, the number not being considerable, it is much easier to study the system, and to render it always sufficiently present to our minds, to apply it easily as occasion may require, particularly when on the one hand the progress is traced according to fixed principles, which second the efforts of the memory by connecting it with the understanding, and when on the other hand the methods which it employs for characterizing the bodies belong to interesting observations or experiments, which leave upon the mind durable traces of what has once spoken as it were to the eyes.

We have collected under one and the same point of view the principal characters which may serve for the description of a mineral, and we have formed a table of them, which will be found prefixed to the plates. We have arranged this table according to the methodical order of the different branches of knowledge to which it refers; this order having appeared more favourable for assisting us in seizing the whole at a glance, and to render it present to the memory.

We shall here subjoin a series of annotations, intended to give a more developed idea of certain characters, or details relative to the method of verifying those which, in order to become evident, require experiments.

These annotations will be followed by several distinct tables, which will present successively the indication of the specific gravities of minerals reduced to their limits, that of their hardness, the enumeration of the substances which possess double refraction, of those which are electrical by heat, of those which have for their primitive form a rhomboid, an octahedron, or a solid of another kind, &c. These various tables will serve as a kind of supplement to the system, particularly with respect to the second class, which

which has remained without subdivisions; or rather they will form by themselves a kind of system, which will have the advantage of multiplying the points of view under which minerals may be regarded.

ANNOTATIONS RELATIVE TO THE GENERAL TABLE OF MINERALOGICAL CHARACTERS.

Physical characters.

1. *Specific gravity.* Let us suppose a series of bodies of different natures, which have equal volumes. If we weigh all these bodies successively by means of common scales, it will be necessary, in order to establish the equilibrium, to employ weights more or less considerable, according as these bodies are more or less dense. Let us suppose, moreover, that, having taken as a term of comparison one of these bodies, for instance the lightest, we represent its weight by unity, and we express the weight of all the other bodies by numbers proportional to that unity. We shall have the relations between the weights of the different bodies of equal volume, or the specific gravities of these bodies.

But the hypothesis, that all the bodies of which we would propose to determine the specific gravities have equal volumes, not being capable of being realized, it will become necessary to seek for another method in order to attain the same object. We might succeed equally well, were we to estimate exactly the volume of each body; after which it would be easy to bring the results of the different weighings to what they would have been on the hypothesis of there having been an unity in the volume. But as this method presents obstacles which are insurmountable in practice, we supply its place by an ingenious process, which consists in seeking for the relation between the weight of each body when weighed in the air, and the loss of weight which appears when the same body is weighed in water, which we here suppose to be respectively lighter than it. This loss proceeds from the effort made by the water to sustain the body in part; and this effort being equal to that which it exercised in order to keep in equilibrium the volume of the same liquid displaced by the body, it results that the loss in question represents the weight of a volume of water equal to that of the body. We have therefore the relation between the weight of the body and that of the water in equal volumes; and this liquid thus serves as a common measurement, in order to compare with each other the specific gravities of the different bodies.

It

It is in this way that the tables of specific gravities published by several authors have been made up. Of these the table of M. Brisson is the amplest and the most correct.

We employ in these experiments the *hydrostatic balance.* The body to be examined is suspended by means of a horse-hair to a small hook under one of the scales, which procures the facility of plunging this body into water in order to weigh it.

Nicholson has suggested in these experiments the use of an areometer of tinned iron, represented in fig. 75, and the stalk of which, B, is a brass wire, which has at its extremity a small cistern A. This stalk is marked in the middle by a line *b* made with a file. The lower part keeps suspended an inverted cone EG, concave at its base, and balanced within by a piece of lead*. The weight of the instrument ought to be such that, when we plunge it in water and leave it to itself, a part of the tube floats above. The cistern at the top of the stalk, and which has the form of a spherical shell, is fixed to it by means of a small tube of tinned iron into which this stalk enters. Generally there is a second cistern somewhat larger, which is placed above the first, into the concave part of which it is fitted, in consequence of its convexity. We may thus move this second cistern, either to remove more easily the weights with which it is charged, or to make some change in their arrangement. This instrument, which is very cheap, and easily carried from place to place, is extremely useful to mineralogists. An example will best explain the method of using it.

If any doubts are entertained whether a transparent stone of a blue colour belongs to the stony substance commonly called *oriental sapphire*, or to the variety of quartz which is called *water sapphire* :—Take distilled water at a given temperature; Brisson has adopted that of 14° of Reaumur, which answers to 17·5° of the centigrade thermometer, as the medium in our climate. Having plunged the areometer in this water, charge the upper cistern A, until the scratch *b* marked on the stalk descends to the level of the water. This is called levelling the areometer.

* In several areometers the cone has a fixed position, by means of brass wires which keep it attached to the instrument. But M. Gillet with good reason prefers giving it its liberty, by suspending it with a hook, as is represented in the figure. In this way the axis of the instrument always takes a vertical direction, otherwise it would lean more to one side than to the other, when the instrument would be in equilibrium round its centre of oscillation.

areometer*. Let us suppose that the weights employed form a sum of 20 grammes. This is the charge of the areometer, which can only serve for bodies the weight of which does not exceed 20 grammes.

Having taken out the charge, put the stone into the same cistern, and place beside it the weights necessary for levelling the areometer. Supposing these last weights to be equivalent to 13·6 grs. subtracting this number from 20, we shall have 6·4 grs. for the weight of the stone in the air.

Withdraw the areometer, in order to place the stone in the lower bason E; then having redipped the instrument, add into the cistern A the weights necessary for producing the level once more. Let us suppose these additional weights to be equivalent to 2·48 grs. This is what the stone has lost of its weight in the water, and at the same time is the weight of an equal volume of water.

Make this proportion: 2·48 grs. or the weight of the volume of water equal to that of the stone is to 6·4 grs. the absolute weight of the stone, as the unity which represents in general the specific gravity of the water is to a fourth term, which will be the specific gravity of the stone†. This fourth term taken with four decimals is 2·5806. Now on running over the table of specific gravities, we find that that of water sapphire answers to nearly the same number, while that of the oriental sapphire is about 4. The stone subjected to the experiment is therefore a quartz only.

If we wished to weigh a substance respectively lighter than water, it would be necessary, on placing it in the lower basin, to fix it steadily to it. In this case the body which serves to fix it is considered as forming part of the areometer. In other respects the operation is the same as in the preceding case; only the second term of the proportion is smaller than the first, which is necessary; since the fourth term, which gives the specific gravity of the body, ought to be also smaller than the third, which represents the specific gravity of the water.

* We may fairly dispense with this operation, because we are understood to know before-hand, from a prior experiment, the weight necessary for levelling the areometer: it is best however to repeat this operation every time, on account of the small differences which may happen in the temperature, or in the quality of the liquid.

† It is more natural to employ unity in order to designate the specific gravity of the water, which is the term of comparison to which we refer all the specific gravities of other bodies, than to represent it by 1000 or by 10,000, as is generally done. To conclude, the calculation is the same, except that we have generally a decimal fraction in the result.

Let

Let us suppose, for example, that the absolute charge of the areometer, including the body which ought to serve as the means of fixing the substance, being also 20 grammes, we must have been under the necessity of placing 16 grammes beside the body which we wish to weigh, in order to produce the levelling once more. We shall have four grammes for the weight of this body. Let us afterwards suppose that, the same body being dipped in water, we had still added six grammes to the 16 which were already in the upper cistern, which makes in all 22 grammes. These six grammes will represent the weight of the volume of water displaced, and the proportion will be $6 : 4 :: 1 : x$, which will give 0·6666 for the specific gravity of the body subjected to the experiment.

In effect, if the weight of the body of an equal volume was exactly the same with the weight of the water, it would be necessary to charge the upper cistern with 20 grammes only, as at first, when the body would be plunged in the water, because it would perform the office of the volume of water displaced. But we have seen that in this case the upper cistern was charged with 22 grammes, from which it follows that there remains to the water an effort of two grammes, besides that of four grammes, which it employs in sustaining entirely the body. The total force therefore of the water is equivalent to six grammes; or, what comes to the same thing, the weight of a volume of water equal to that of the body is six grammes. Thus the specific gravity of the water is to that of the body in the ratio of six to four, as we have seen above.

Water always has a trifling adherence to the areometer, which is such that this instrument loaded with the same weight may remain a little more or a little less deeply plunged in the water. In order to get rid of the trifling uncertainty which arises from this variation in the level, having allowed the areometer to attain the state of stability, raise it a little above its position, and afterwards sink it a little below it by abandoning it every time to itself; and if the scratched line is between the two points which are levelled, you may conclude that the upper basin has its true charge.

We may instead of distilled water employ rain-water, which at the same temperature has visibly the same density. In the event of our only wishing to dispel a doubt whether a mineral might be referred to one species more than to any other, we should have a sufficient approximation on operating with river- or well-water, the temperature of which should differ in a few degrees only from that which had been chosen for arranging the table of specific gravities.

gravities. If we should desire, however, a greater precision, we might attain it by the method which we shall indicate, in order to assimilate the weight made with any liquid at any given temperature, to the result which would have been given by distilled water at 14° of Reaumur.

Having taken precisely the absolute weight of the areometer, which will be, for example, 152 grammes, and knowing the additional weight supposed to be 20 grammes necessary for levelling it in distilled water at 14°, we shall have for the sum of these two weights 172 grammes.

Let us now suppose that the additional weight which produces the levelling with another liquid is 20·5 grs. the sum will become 172·5 grs.

Now we know that when a body floats in part, the weight of the volume of the liquid which answers to the part immersed is equal to the total weight of the body. Thus, since the part immersed is the same in both cases, it results that the weights of the two liquids when of equal volume, or what comes to the same thing their specific gravities, are in the ratio of 1720 to 1725.

This being done it is evident at first sight that the liquid substituted for distilled water immediately gives the absolute weight of the body under examination, without any correction being required. Let this weight be eleven grammes. After having found by a second operation the quantity which the body, weighed in the liquid which you employ, loses of its weight, and which we shall suppose to be 4·7 grs., make this proportion, 1725 : 1720 : : 4·7 : a fourth term which will indicate the loss corrected, or that which the body would have undergone of its weight in distilled water at 14° of Reaumur. This loss, which will be found to be 4·69, will give at the same time the weight of the volume of distilled water at 14°, equal to that of the body; after which you will make this other proportion which returns to that above indicated 4·69 : 11 : : the unity is a fourth term, which will be 2·3454, and which will indicate the true specific gravity of the body. By employing no correction, we should have found 2·3404.

There are substances which being immersed in water drink up this liquid. Of this number is the mesotype (zeolite of Cronstedt). We perceive this property when, having placed the substance in the lower basin E, we see the areometer descend, after having mounted up, although the cistern A remains loaded with the same weight. In this case we shall allow the body to imbibe the whole quantity of water which it can admit into its pores, and we shall know that it has reached this kind of point of saturation

tion when the areometer remains in a fixed position. Then we shall level it; and we shall in general seek for the loss which the body has undergone of its weight in the water. We shall afterwards seek for the weight of the quantity of water which it has imbibed, on weighing it again in the air, and in subtracting the first weight from the second; then we shall add the difference to the loss formerly found: and the result will give the true loss, or that which would take place if the body was not susceptible of imbibition; after which we shall form the proportion indicated above.

Let us suppose, for example, a mesotype, the weight of which in the air, is nine grammes. Let us suppose that the loss of weight which it experiences in the water, after imbibition, is 4·3 grs. Let us suppose in fine, that being again weighed in this state, it gives 9·13 grs. Subtracting the first weight from the latter, we shall have 0·13 grs. for the quantity of water which the mesotype has imbibed. The real loss, or that which the substance would have experienced of its weight in water, if it was not penetrable by this liquid, will therefore be 4·3 grs. plus 0·13 grs. or 4·43 grs.; which gives the following proportion, 4·43 grs. : 9 : : 1 : x. From which we shall conclude that the specific gravity is 2·0316.

In fact, since bodies lose less of their weight in water in proportion as their absolute weight is more considerable, it results that the mesotype ought to have lost in the water 0·13 grs. less than if the imbibition had not taken place. We must therefore add 0·13 grs. to the loss found by experiment, in order to have the loss corrected.

The character which is drawn from the specific gravity joins to the advantage of a great generality that of being susceptible of a precise estimation, provided we do not employ too small pieces of any substance. Its limit relatively to each mineral is the result of the operation made on a piece chosen in the greatest possible state of purity. It may vary beyond this limit on account of some colouring principle of a metallic nature; or within this limit, by the effect of the mixture of a substance less dense, or whose presence relaxes the aggregation of the molecules. Thus the specific gravity of the limpid hyalin-quartz, called *Madagascar crystal*, is 2·653; that of the red hyalin-quartz is 2·6701: and that of the dull quartz is only 2·6459. The more do the limits relative to the species between which it is requisite to determine escape being confounded, the more decisive is the proof of the character.

2. *Hard and soft bodies.* The character suggested by hardness

hardness does not admit of quite so much precision as the foregoing. It is besides more variable. Some particles of quartz, disseminated in a body of a soft nature, may, by altering a little its specific gravity, make it capable of striking fire with steel. But this character has the advantage of being easy and expeditious in its application, by the help of the different methods pointed out in the table. It is proper to distinguish accurately the species whose varieties are but little subject to the alterations produced by accidental mixtures. Thus the cymophane, or the pearly chrysolite of the lapidaries, scratches glass, whereas the chrysolite of Romé de l'Isle, since ascertained to be a calcareous phosphate, when rubbed upon glass, frequently leaves a white mark of its own substance. The ferruginated schéelin or wolfram yields very easily to the file, which has much less action on the blackish oxidized tin sometimes found in the same matrix, &c.

3. *Brittle bodies.* These must not be confounded with what are called *soft* or *tender*. Talc is softer than carbonated lime, denominated *calcareous spar*, since this last scratches it. But it is less brittle, as it resists percussion far better.

4. *Elastic bodies* are those which, after having been compressed, return of their own accord to their original figure. The character suggested by this property is not employed in mineralogy, except when speaking of bodies which appear to us in the form, or may be easily reduced to the form, of thin laminæ or filaments, and it suits but a small number of species.

5. *Ductile bodies* are those which are lengthened or flattened by percussion or by pressure, so as to preserve the figure which they have taken in virtue of the one or the other of these two forces. Of this number are several metallic substances, gold, silver, copper, &c. Whereas others, such as antimony, bismuth, &c. will break, rather than submit to be flattened or elongated.

6. *Bodies endowed with tenacity.* This is the property possessed by certain substances, and particularly by the metals, of resisting without being broken the action of a force which draws them by one extremity while they are fixed at the opposite extremity. Of this kind is the resistance presented to breaking by a harpsichord string which we are tuning. This property is but little susceptible of being employed as a character, but it is right to mention, in the description of a mineral, to what degree it possesses it in comparison with others.

7. *Adherence to the tongue.* Certain bodies adhere to the

the tongue when brought in contact with it, and a slight resistance is experienced when we separate them. This effect is produced by the faculty which the body has of absorbing the saliva which moistens the tongue, and thus bringing the substance into more immediate contact with this organ. If we put a drop of water on one of these bodies, we shall observe that it is imbibed in an instant, and this proof may suffice instead of applying it to the tongue.

8. *Colours.* In order to fix the degree of confidence which these characters deserve when borrowed from this modification, too much neglected by some, and overvalued by others, we ought to consider it in two very different points of view, according to the various natures of the bodies which are furnished with it. In a certain number of these bodies, and in particular in the earthy and acidiferous substances, the colours are owing to the molecules of a foreign principle, which is frequently iron, and sometimes chrome or manganese, disseminated among the molecules peculiar to the coloured body. Hence it happens that one and the same substance, for example fluated lime, is colourless in certain pieces, and in others presents alternately the red, yellow, green, violet, &c. In this case the colour is only a transient accident, which may merely serve to distinguish certain varieties.

But in other minerals, such as metallic substances, sulphur, amber, some saline substances, the reflection of the rays which produce the colour is made upon the proper parts of the coloured body; it depends on its texture and on the degree of the tenuity of its molecules. It may then be ranked among the specific characters.

We sometimes find muriated soda coloured red. If you dissolve it in this state, it will be stripped of its colouring principle, and the new crystals which it will form will no longer reflect any thing but white light. The operation only frees it from a superfluity, without which it does not cease to be of the same nature. On the contrary, sulphated copper subjected to the same experiment, as often as we please, will always reappear blue, because this colour is inherent in its nature.

Thus those who have said that the true colour of the spinel ruby, for instance, was red mixed with orange, have only designated the stone which pleases amateurs most. But to say that the true colour of gold is pure yellow, is to speak the language of the naturalist. If this colour does not uniformly exist in gold, this must arise from the pre-

sence

sence of a foreign substance which has altered the metal itself.

I have met with naturalists, however, who, admitting the variations to which the colour is subject, with respect to one part of the minerals, do not warrant us in attaching much importance to the character which it furnishes; as much because it is that which first speaks to the eyes, as because there is always, according to them, relatively to each substance, a colour which is predominant, and which agrees with the greatest number of varieties. But the more the observations shall be multiplied, the more frequently will it happen that this character will not speak to the eye, except to deceive it and make it take the change. No further proof of this is requisite than what happens with respect to the emerald. Grass-green seems long to have been ranged among the general characters of this substance, and, in short, it is not astonishing that every thing which was emerald was of the same colour. We were not acquainted with any thing else, properly speaking, under this name, than the crystals from Peru, which being formed in the same circumstances had received the impression of the same colouring principle. A discovery in which mineralogy and chemistry have concurred, unites the beryl with the emerald; and from this moment we have emeralds, some greenish-yellow, others blueish, and others of a decided yellow; and the number of the crystals of these different tints, particularly of the first, which exist in our collections, far exceeds the ancient emeralds. The jargon of Ceylon identified with the hyacinth, according to the analysis of Klaproth, has also disturbed this last substance in the place it occupied, namely, that of being exhibited of an orange brown colour only, excepting in one variety which was whitish: and it would be as easy to adduce other examples of this kind *, as it is to foresee from the moment that these examples will still continue to be multiplied. The indication of the colour, in the case of the latter being owing to an accessary principle, ought therefore to be dismissed from the specific character; and we shall have a new reason to exclude it, if we consider that every thing

* Launoy brought from Spain several small orange crystals which belong to phosphated lime, known in Germany by the name of *spargel-stein*, because the colour of the variety of this substance found at first in the same country inclines to that of asparagus. Messrs. Abildgaard and Manthey have since given me some crystals of this substance which are frequently met with in certain granitic rocks of Norway, and the colour of which is sometimes of a greenish blue, sometimes brown, &c.

which

which enters into this character ought to be so much the more taken into the account, the purer the substance is, or the more it approaches the limit which really constitutes its species: and that in the case of this limit the colour would disappear*.

As to the diversities of which the character is susceptible that is derived from colours, it would be superfluous to enumerate them, because we find on this head in daily observation, and in the commonly received language, every thing that can be required by science and its nomenclature.

Thus, in order to designate any given shade of colour, sometimes we add a simple adjective; as when we say *pure green, bright red, dark blue:* sometimes we refer the colour to a term of comparison taken from among familiar objects; as when we say *sky blue, saffron yellow, leek green,* &c.: on other occasions we give the two colours of which the object in question seems to partake, and say, for example, *greenish yellow,* or *yellowish green,* by calling in the predominant colour first.

9. *Cat's eye colours.* This expression alludes to the eyes of a cat which shine in the dark. We say of a substance that it is cat's eyed, when, in proportion as we vary the position of its surface, the reflections of light which it gives off are in some measure moveable, or appear and disappear alternately.

10. *Metallic lustre.* We may distinguish the true from that which is apparent only, in so far as the mark of a file or any sharp instrument with which we have scratched a metal does not cease to shine, whereas it is dirty, and as it were powdery, when the body is not of a metallic nature.

11. *Limpid bodies.* We have given the name of *white* to diaphanous and colourless minerals properly so called, such as the quartz called *Madagascar crystal.* We shall call these *limpid* minerals, and reserve the deno-

* We must confess that there would have been some advantage in quoting at the head of the description of a species, the character derived from the modification in question, if the greater number of the varieties presented one and the same colour, or nearly so, in such a way that the differences which would have taken place in other varieties might have been regarded as exceptions; because, this character being that which first strikes the eye, its indication would thereby be very proper for becoming as if the first stroke of the pencil in giving the portrait of a mineral But if we are obliged to quote at once eight or ten different colours, which are shared by various individuals of the species, will it not appear that the description commences by falling into absurdity, and by failing in its principal object, which is to admit of a facility of ascertaining at one glance the substance indicated?

 mination

mination of *white* for those which reflect without order the assemblage of all the colours, like statuary marble.

12. *Double refraction.* When a ray of light passes obliquely from one medium into another of a different density, it is diverted from its route, forming a kind of fold. This deviation, which we call *refraction*, is subjected to a constant law which is known to all naturalists.

Certain substances have the singular property to solicit the ray which penetrates them to divide itself into two parts which follow two different routes. This is called *double refraction.*

When the refraction is simple, we only perceive a single image of an object seen through two faces of a transparent piece of the mineral employed on this occasion, whereas, if it were double, we might in the same case see two images of the object. But in order to obtain this effect with most of the substances endowed with the property in question, we must choose two faces inclined towards each other, whether we employ a crystal given by nature or a piece cut by the lapidary.

The quantity of double refraction, or, what comes to the same thing, the opening of the angle formed between each other by the rays, by means of which the eye sees the two images, varies from one substance to the other, every thing else being considered according to the nature of the substances themselves. In zircon, for instance, the double refraction is very strong, whereas it is much less perceptible in the emerald. Besides, this quantity varies in every substance, from various causes. In general it increases or diminishes, according as the refrangent angle, or that which is formed between each other by the two faces, through which we view objects, is more or less open. But there is another cause of variation, which is combined with the foregoing, and which depends on the position of the refrangent surfaces relatively to the faces of the primitive form; and such is the influence of this cause, that under two equal refrangent angles differently situated, we may have distances evidently unequal between the images of the same object, and there is even a limit at which the effect of the double refraction becomes null, *i. e.* the two images are then confounded into one.

This limit takes place, for instance, in the quartz and in the emerald, when one of the faces which belong to the refrangent angle is perpendicular to the axis. It takes place in sulphated barytes, when one of the same faces being parallel

parallel to the axis, is at the same time parallel to a plane which should pass by the great diagonals of the bases of the primitive form. I have but a very few observations on this subject as yet; but it is probable that all the substances which have the double refraction fall within one or other of the foregoing cases, which give of themselves the limits of all the positions which refrangent surfaces may have relatively to the primitive form. But as the position parallel to the axis is variable in its turn among several limits, which correspond with the diagonals and the sides of the bases of the primitive form, it will be requisite to know which of these last limits is that which agrees with every substance.

I shall explain, when I come to the article emerald, how a mistake led me to these results; and I even confess that I am still in uncertainty as to the refraction of some substances, not having had time sufficiently to multiply my inquiries, in order to ascertain if a crystal of this description which presented only a single image of the objects, would not exhibit two after having been cut in a certain manner.

I shall detail, in speaking of every substance, what I have observed with respect to its refraction, and I propose to make some fresh experiments on this delicate point connected with minerals, which I have been only able to glance at as yet. We shall find, under the article carbonated lime, the detail of the particular results which I attained relative to the double refraction of this acidiferous substance, which suits more easily than the others this kind of inquiries.

Another observation, which will not be altogether useless when we are occupied with generalizing the theory of the phænomenon in question, consists in this—namely, that all the substances in which the integrant molecule is remarkable for its symmetry have the simple refraction. Of this kind are those which have for their primitive form the cube, the regular octahedron and the rhomboidal dodecahedron.

As yet we have only subjected to the experiments which concern this object, bodies taken from among those which we commonly designate by the name of *stones*. I have extended these experiments to several of those which are called *salts*, as well as to inflammable substances, and to metallic substances oxidized and united to other principles, and I have found that there is no class of minerals which does not present bodies endowed with double refraction.

 There

There are various ways of observing the double refraction. One of the most simple consists in taking a pin by the point, and presenting it against the window at a certain distance from the eye, against which we shall keep at the same time the mineral applied by one of its faces. By making the pin assume various positions, we shall find that there is one in which we see two distinct images of this pin parallel to each other, and generally prismatic (*irisées**). Then, if we gently turn the pin until it is perpendicular to its first position, we shall see the two images approach by degrees, until they fall upon one and the same line, in such a manner, however, that one of the two heads will frequently exceed the other. We may also make use of a card on which we have traced a line with ink of a good tint.

The separation between the images is more sensible, the distance between the object and the eye and all other circumstances being alike, when the diaphanous body used in the experiment is of a greater thickness. And if we suppose this thickness, in its turn, to be constant, and the object removed from the eye, the two images will be more and more removed from each other, at the same time that they will be diminished in distinctness.

The following is a third advantageous process for short-sighted people. Place a lighted candle at a certain distance in a dark room. Having afterwards made a hole in a card with the point of a pin, apply it to one of the faces of the stone, so as to make the hole correspond to a point of this face; then having approached with the eye the opposite face, seek the position proper for enabling you to perceive the flame of the candle. You will then have the two images distinct and well defined, because the effect of the hole made with the pin is to dismiss the kind of irradiation which dazzles them, when we employ the stone by itself.

It would be difficult to find a character more prominent than that which is drawn from the double refraction, since it belongs to the very essence of the minerals in which it exists. But we cannot always observe it on taking these bodies in the natural state. Several require to be prepared

* When the double refraction is not considerable, it may happen that the two images touch each other. But, upon attentively examining the head of the pin, we can distinguish at this place as it were two small circles which intersect each other: and besides, we shall observe that the same colour which edges on one side the prismatic band reappears on the line of the middle part, where the same series recommences.

for

for inspection by cutting. Those which are called gems, and which have come through the hands of the artist, thereby become susceptible of presenting the effect of simple or double refraction, when we know how to guard against the illusion produced by the multiplicity of the facets; and it is even an advantage to be able by the help of this multiplicity to vary the refrangent angle, because, if any one of the facets was situated in the direction of the limits where the two refactions are reduced to one only, other facets would present themselves in order to dispel the doubt. We shall thus avoid confounding a piece of crystal of Madagascar with the gem called by lapidaries *white sapphire*, their Brazilian ruby with the *balass ruby*, the topaz of Saxony with what is called *oriental topaz*; the first stone of each of the above pairs having the double refraction, while the second has the simple: it is fortunate to be able in these cases to make up for the disappearance of the crystalline forms by a physical observation, and to read in a manner into the interior of the stone, when its exterior speaks no longer to the eye.

13. *Phosphorescence by the action of fire.* In order to observe this character, we must throw on red-hot charcoal a small quantity of the dust of the mineral we wish to examine. The phosphorescence in question is not simply a scintillation, like what is produced by the sawings of wood thrown upon the flame, but a mild and agreeable light similar to that of the glow-worm, the tone of colour excepted, which varies in different substances. This experiment scarcely ever succeeds except in the dark. We must also take great care to pound the mineral well, lest any decrepitation might throw the fragments into the eyes of the by-standers.

14. *Electricity.* There are three ways of exciting the electrical virtue in bodies; namely, by friction, by communication with a body already in a state of electricity, or by heat. This last method takes place only with respect to certain mineral substances.

We distinguish two kinds of electricity: the one which we call *vitreous*, and which Franklin called *positive*, is that which friction produces in glass and other vitreous substances. The second, which we call *resinous*, and which Franklin described by the name of *negative*, is that which is acquired in the same case by resin, sulphur, silk, &c.

These two electricities exercise contrary actions; so that two bodies, both of which are solicited by vitreous electricity or by resinous electricity, are repelled, whilst two

bodies one of which possesses the vitreous and the other the resinous electricity are mutually attracted.

Among the number of bodies capable of receiving electricity by friction, we find some, which, after having been simply presented to the fire for a moment, or dipped into hot water, have acquired the electrical virtue. These bodies have in this case one side solicited by vitreous electricity, while the side diametrically opposite gives signs of resinous electricity.

One general observation made on such of the same bodies as are crystallized, consists in this; namely, that their forms take from the symmetry of the ordinary crystals, in the same manner as the parts in which the two species of electricity reside, although similarly situated on both sides, differ in their configuration. The one undergoes decrements which are null on the opposite part, or to which some decrements answer which follow another law. It results that on a simple inspection of one of these crystals, we may indicate beforehand the side which will give signs of vitreous electricity, and that which will manifest resinous electricity.

Electricity separates the whole mineral kingdom into three great divisions, which follow nearly the methodical order generally adopted for the classification of bodies of this kingdom. Almost all the substances known by the names of *stones* and *salts* acquire by friction the vitreous electricity, provided they enjoy a certain degree of purity. The inflammable substances properly so called, with the exception of the diamond, being rubbed in the same manner, receive on the contrary the resinous electricity. The metallic substances possess in general in an eminent degree the conducting property of electricity. A few of them, which being mineralized approach the saline state, such as carbonated lead, also enter into analogy with the salts, by the faculty of acquiring the vitreous electricity by means of friction.

I ought to premise that we here allude to the ordinary methods of exciting electricity; as when we employ the friction of the hand, or that of a piece of cloth. I suppose also that the bodies rubbed are polished; for there are some kinds of quartz, gems, and other analogous substances, such as glass, which acquire the resinous electricity by means of friction when its surface is dull.

It results from all that has been said, that the electrical property furnishes characters useful in several respects for the distinction of minerals.

Electricity

Electricity by communication, employed alone, may serve to discover the presence of a metal mixed in a considerable quantity with a stone, as takes place with the iron which enters into the composition of jaspers. In order to ascertain this character, we insulate the stone on a small stalk, so as to bring it in contact with an electrical conductor, and we judge whether the stone is electrical or not by communication, according as the touch of the finger or the ball of the discharger draws sparks from it.

The electricity by friction observed comparatively in two different stones may assist in distinguishing them from one another. The cymophane when polished but not cut, which presents nearly the same appearance with mother-of-pearl feldspar, called *moon-stone*, differs from it by the great facility which it has of being electrified by friction, whereas the same method succeeds but ineffectually and feebly on feldspar.

The simplest apparatus for experiments of this kind consists of a small copper needle *a b* (fig. 76, A) terminated by two bowls and moveable on a pivot. After having rubbed the mineral several times on a piece of cloth, we must present it to one of the bowls, and we may judge pretty nearly of the strength of the electricity, by the distance at which this bowl begins to be attracted.

With respect to substances electrical by heat, such as the tourmaline, we make use of the same apparatus when we merely wish to ascertain what they are. But it is interesting to be afterwards able to determine the parts in which the two electricities reside. To effect this take a stick of sealing-wax, to the extremity of which a silk thread is attached little better than one eighth of one inch in length: after having rubbed this stick, present by turns the two opposite sides of the substance, for example, the two summits of a tourmaline, at a small distance from the silk thread. If the summit which faces the thread be the seat of resinous electricity, there will be repulsion. In the contrary case the thread will be attracted.

We may vary this experiment, by placing the stick of wax, after having rubbed it, below one of the two bowls which terminate the needle, at the distance of about a quarter of an inch. For the greater simplicity, we may give such a height to the stand of the needle, that the stick of wax when resting by the rubbed extremity on another stick or on a glass tube placed transversely, and by the other extremity on the table which holds the apparatus, is at the distance

distance required for the success of the experiment. In this case, the wax acting on the bowl communicates to it an electricity contrary to its own; whence it follows that we have inverse effects to the foregoing, *i.e.* the side of the stone solicited by the vitreous electricity repels the needle to which we present it, and that which possesses the resinous electricity attracts this needle to it. This method is preferable to the first, when the electrical body is very small, or has but a feeble virtue.

Fig. 76 B represents the experiment described. We there see the tourmaline *tt'* caught by pincers supposed to be held in the hands of the observer, in such a way that the pole *t* is at a small distance from the bowl *a* of the needle. Cc is the stick of wax which rests by one of its extremities on a tube of glass U*u*, and which acts by its part C on the bowl *a*, in order to produce the vitreous electricity.

In what has gone before we have considered the effects of the action exercised on a mineral by another body, so that the former may be regarded as passive with respect to the latter. What we now call *active electricity* is that which the mineral excites of itself in the sealing-wax, by means of friction. In order that the experiment may succeed better, we must, after having heated the stick of wax, flatten it at one end by pressing it on a smooth body. We must afterwards rub this same end with a part of the mineral, which is itself smooth, or at least free from asperities; then we shall present the wax to the copper needle under which we have placed before-hand another electrified stick of wax, as has been already mentioned.

Every body, the friction of which thus communicates to wax a certain species of electricity, acquires at the same time the contrary electricity; so that we might consider this last electricity in preference, or, what comes to the same thing, consider the mineral as being passive with respect to the wax. But the minerals in which this experiment becomes interesting being conductors of electricity, it is simpler to examine their action upon wax, either because without this expedient we should be obliged to insulate them, or because when their volume is somewhat considerable their electricity, by being diffused over a large surface, would not be sufficiently palpable.

We have as yet but a small number of substances which excite the vitreous electricity in wax, whereas other substances of an analogous nature produce in it the contrary electricity.

electricity. These are exceptions as it were to the ordinary results, susceptible even on that very account of accurately designating the minerals which present them.

15. *Magnetism.* We know that two magnetic needles, when they turn towards each other their north or their south poles, are repelled; whereas there is an attraction, if the poles facing each other are one of them south and the other north. In consequence we recognize a needle, *i.e.* a piece of iron in the state of permanent magnetism, from the same side of this piece of iron presented successively to the two poles of a magnetic bar suspended freely, attracting the one and repelling the other, or *vice versâ*.

But if we employ a piece of common iron, there will be attraction in both cases; the pole nearest the iron will communicate to the part turned towards it a magnetism contrary to its own, so that there will then be two magnetic needles which will face each other by opposite poles. The magnetism thus acquired is merely instantaneous: it gives place to the contrary magnetism the moment the iron passes from the neighbourhood of one pole to that of the other, and is dissipated the instant the iron is no longer in the sphere of activity of the bar.

In experiments of this kind it is preferable to use a needle in the form of a lozenge, three or four inches long, instead of a bar, the former being more sensible. But the bar would be preferable, if it were requisite for instance to pick out some specimens of iron scattered in a pulverulent mass.

I shall show under the head of oxidulated iron, that most of the crystals, or even the rude masses of this metal, locked up in the bowels of the earth, provided they are not too much oxidized, are two magnets, but of which we cannot observe the polarity except by using a needle slightly magnetic, and this for a reason which I shall give at the same part of my work.

[To be continued.]

LVII. *Account*

LVII. *Account of the Whynn Dykes in the Neighbourhood of the Giant's Causeway, Ballycastle, and Belfast: in a Letter to the Lord Bishop of Dromore, from* WILLIAM RICHARDSON, *D.D. late Fellow of Trinity College, Dublin**.

MY LORD, WHEN I last had the honour of conversing with you on basalt subjects, you were surprised when I told you that the *whynn dykes*, which of late have so much occupied the attention of naturalists in the western isles of Scotland, originated on our Irish coast, and especially about the Giant's Causeway.

As your lordship expressed a wish for further information of the subject, I promised to communicate to you such observations as I should make when I had examined the coast a second time, in order to ascertain the *facts* with the utmost precision.

Previous to my entering into a particular account of our dykes, I will take the liberty of making a few general observations on those in both countries.

The whynn dykes in the Hebrides are seen under very different circumstances from those on the northern coast of Ireland. There they are found on, and above, the surface, generally a few feet; and often serve as fences, whence they obtain their name. In this form they run northwards quite to the extremity of these islands, ascending and descending mountains, crossing seas; and where these are narrow, the dykes that run into the water at one side of a channel, are seen rising out of it at the other side, steadily pursuing their formed rectilineal course.

With us they are sometimes exhibited in a very different manner. Their first appearance is in the faces of our vast perpendicular precipices, where they are seen cutting vertically the several strata of which these are composed, and then burying themselves in the northern ocean.

The observations made on these whynn dykes in the two countries, taken together, make our information on the subject complete. In the Hebrides we are surprised at the incredible length to which these mighty walls proceed,

* Dr. Richardson's paper on the Basaltic Country in the Counties of Derry and Antrim, published in our 33d vol. has excited so much attention, that I am persuaded I shall render an acceptable service to geologists by giving in the Phil. Mag. some previous papers by the same gentleman, published in the Transactions of the Royal Irish Academy. The present paper is from their 9th volume.—EDIT.

and

and we see them penetrating indifferently all substances they encounter:—with us we can measure a part, and a part only, of their stupendous height, as at the Milestone 100 feet, at Port Spagna 330 feet, at Fairhead probably more; and we can observe the effect, or rather the non-effect, produced at their contacts with the different materials they meet, as they are seen in the faces of our precipices.

By Mr. Mills's account, (Phil. Trans. 1790,) the island of Lismore, entirely limestone, is crossed by whynn dykes, as is the limestone at Gartness; at Iona granite is the contiguous matter, at Juva chert, at Persabus a whynn dyke is crossed by a lead vein, and another at Glascow Beg; at the isle of Arran Mr. Jameson finds them cutting through porphyry and micaceous schistus.

With us the whynn dykes at the westward of the Giant's Causeway cut through strata of table basalt, and red ochreous matter, placed alternately; at the Giant's Causeway, and Port Spagna, they cut through strata of finer basalt, disposed in prismatic pillars; while at Fairhead they encounter new materials, to wit, alternate strata of freestone and coal.

In both countries these mighty walls are always of basalt; their general thickness is from twelve to fifteen feet, though in one or two instances they do not exceed two or three feet, and at Gartness the whynn dyke is 23 yards across; but it has not been ascertained in any instance to what depth they reach beneath the surface, even in the deepest mines.

Though the material of which these walls are composed seems to be in general the same, yet from Mr. Mills's account there are important differences between the Scotch whynn dykes; and with us scarce any two of our dykes, that are accessible, exactly (as will appear) resemble each other.

As the whynn dykes Mr. Mills observed are unquestionably basalt, he calls them all lava, and attempts to prove it by a sort of vague induction: page 75 he says, Islay whynn dykes resemble those at Ballycastle, *which take their rise in a country confessedly abounding with volcanic matter.*

Now the specimens from the Islay dykes strongly resemble (as he says) the Derbyshire toadstone, formed, as he asserts (page 98), by subterraneous fire.

Of Derbyshire I will not presume to say any thing, having never visited it; but the proof of its strata being lava rests upon the admission of Mr. Whitehurst's position, that

that these toadstone strata were formed by successive eruptions of a volcano at the centre of the earth, which pouring up repeated torrents of liquid lava, these spread when they approached the surface of the earth at different distances, and formed the toadstone strata.

When Mr. Mills endeavours to establish his opinions by assertions relative to my country, I will venture to reply to him.

The precipice from which the whynn dykes issue at Ballycastle, by his own account, consists of alternate strata of freestone and coal, not very like volcanic matters: and as to his positive and general assertion, that our basaltic country *confessedly* abounds with volcanic matters, I must reply in his own style, positively and generally, that it does not afford a single particle of volcanic matter; that I have examined this tract for a longer time, and probably with more attention, than any other person ever did, or I would not presume to hazard the assertion so confidently.

When your lordship is so good as to perform the promise you made me, of spending some time with me at the Giant's Causeway, you will be able to judge for yourself as to the truth of these contradictory assertions.

The advocates for igneous operations over the surface of our globe are so prejudiced, that it is sometimes sufficient to refute them merely to quote their own words. As Mr. Mills's paper is now before me, I will give your lordship an instance: He says, (page 98,) "In short, from the very rude and irregular appearance of the summit of the hill (Loffit hill), from its rising so suddenly from the limestone strata, and from the whynn dyke that runs through it, I am strongly inclined to believe it of volcanic origin." Now, as limestone and volcanic matters are not very congenial, and as we do not find that a whynn dyke has been met with in the neighbourhood of any volcano, I conceive, with great deference to Mr. Mills, that if he was determined to draw a conclusion from these data, it should have been a contrary one.—But it is time to proceed to facts.

The westernmost whynn dyke I have met with on our coast, is near what is called the Black Rock, at the end of the Bush Strand. The perpendicular precipice is there not very high (probably 60 feet); it is composed of horizontal strata of table basalts, separated from each other by red ochreous layers.

The dyke (which is inaccessible) is seen from the water to cut all these strata vertically, each of them being interrupted

rupted in its course by this wall, and resumed on the other side of it, precisely at the same level

The second dyke is three or four hundred yards further on, towards the north-east; it is a much finer one, and so happily marked that it cannot be mistaken.

A solitary rock, about 200 yards distant from the main, and visible from a great part of the coast on each side, is called the *Milestone*, from its supposed distance from the Giant's Causeway, but in reality it is much nearer to it. The precipice here has considerably increased in height, being near to 100 feet, accurately perpendicular, and stratified as at the other dyke.

This second dyke reaches from the summit to the water, beneath which we can see it continued northwards, until it reaches the Milestone, which is a part of it.

Though this dyke be also inaccessible, it is plainly formed of prisms laid horizontally, and extending quite across; its thickness seems to be about twelve feet.

The strata are interrupted here, and resumed again, without disturbance, at the other side, as before; nor in either case does the slightest separation appear where these dykes meet the contiguous strata, all forming one solid mass.

The third dyke is situated near the western point of the bay, by which we begin to descend to the Giant's Causeway; of this an isolated fragment alone remains, about 100 feet long by 50 feet high; like the rest it is composed of rude prisms laid horizontally.

Our fourth dyke is at the Giant's Causeway itself; it divides vertically part of the cliff, at the foot of which the causeway is situated and descends quite down to it.

The precipice is not perpendicular here, as at the other dykes, by which means our view of this one is partially interrupted; there is, however, enough of it laid bare to ascertain its nature beyond a doubt, and especially as it is composed of horizontal prisms, a property that seems essential to all whynn dykes.

Where this dyke divides the upper part of the columnar stratum which forms the Giant's Causeway, the basalt pillars on the west side of it have fallen from their original vertical position, until they lean forward almost horizontally; while on the east side of the wall they stand steadily vertical.

The basalt septs, which frequently divide the strata in mines, and appear to be of the same nature with our whynn dykes, are generally attended by a sinking or subsiding of the strata on one side of them, without disturbing the

parallelism

parallelism of these strata. This too is the case with our own whynn dykes at Fairhead; but of the six dykes at Bengore promontory, this fourth is the only one where any thing like a subsiding or depression of the strata can be observed.

This dyke is so accessible, that we are enabled to examine its material and internal construction, from which we are precluded in the former cases; the basalt of which this is composed, though contiguous to, or rather mixed with, the Causeway-pillars, is very different from the Causeway basalt; it is somewhat coarser, more granular in the fracture, and though darker than the gray whynn-stone of the Fairhead pillars, it resembles their colours, more than the fine blue of the Causeway basalt.

The Causeway dyke is 15 or 16 feet thick, sometimes quite solid, sometimes shivery; it is entirely composed of small trapezoidal prisms, their sides about an inch each, and their axes horizontal; they are strongly agglutinated together; and when this wall is attacked by the sledge, it sometimes breaks into fragments composed of an accumulation of the smaller prisms, abundance of which are scattered about the foot of the precipice.

The fifth dyke is at the eastern point of the semicircular bay, of which the Giant's Causeway forms the western point; it is inaccessible, and visible only from the water; it cuts vertically three or four strata of table basalt, also a great stratum of red ochreous matter, and is then lost in the precipice *.

* When I discovered this whynn dyke in the year 1801, I was prevented from examining it accurately by a heavy surf, which deterred me from venturing among the sunken rocks at the foot of the precipice; the next summer I was more fortunate, and enabled twice to reach the bottom of the cliff, where the dyke immerged into the water perpendicularly.

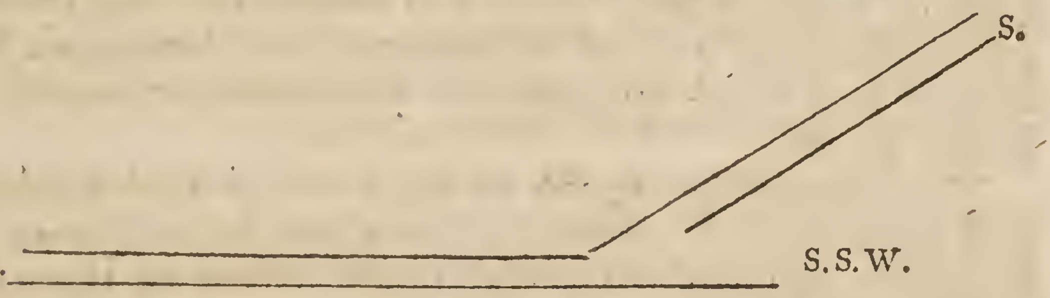
I traced it downwards as it cut the horizontal strata of table basalt vertically, and observed each of these merging into its solid mass without any the least separation of the material; each stratum, having then as it were passed through the dyke, resumed its former position on the other side at the same level it held before, about forty yards from the place where the dyke immerged in deep water; it arose again ten or twelve feet above the surface, continuing its course due north for thirty yards, exactly like a wall, showing the horizontal prisms of which it was constructed, whose bases formed the surface of the wall.

The most curious part of this dyke is discovered by tracing it up the cliff, whose summit it reaches a little to the eastward of its original course; here it projects boldly from the face of the rock like the rectangular corner of a mighty wall about twenty feet thick: yet this curious wall is not entirely dyke, but only its west side, which, at its termination, shows the horizontal prisms composing it; the east side is formed by a range of vertical pillars fifty feet long, part of a great columnar stratum which the dyke there cuts through.

The

The sixth whynn dyke is at Port Spagna, the third semicircular bay east from the Causeway; this is the only one of our whynn dykes that has ever yet been noticed. Mr. Mills (Phil. Trans. 1790) saw from the top of the cliff *a kind of whynn dyke, which ran into the sea towards the N.N.E*; but he did not go down to examine it, and it is from below only that any observations can be made upon it.

This dyke runs into the sea, like a quay about 20 feet broad, formed of huge black stones; its direction near the water is S.S.W. and its two sides accurately parallel: having proceeded thus about 60 yards from the water, the eastern side deflects a little, forming an obtuse angle, while the western side proceeds further in its former direction; the breadth of the dyke thus increases for a little, but the western side is soon resumed parallel to, and at its former distance from, the other side, and the dyke proceeds now due south: all this is best explained by a figure.



The dyke, after having proceeded a short way in its new

The upper surface of this tremendous wall is easily approached from the top of the hill, and covered with high verdure. I have frequently dined upon it, as fortunately the surface is hollow in the middle, by which the dread of a perpendicular precipice, above 200 feet high, (and on three sides not more than eight or ten feet distant,) is considerably abated; the height of the point of the wall from the sea immediately under it is 320 feet.

I dwell upon this dyke both because it is so easy of access from above, (for even carriages can drive to the edge of the cliff,) and also because it is so happily marked as not to be mistaken: it forms the middle point between the Giant's Causeway and the solitary pillar called the Chimney, or, in other words, the common horn of the two crescents or semicircular bays next to the Causeway on the east side.

I will add an account of another dyke lately discovered by my friend capt. R. O'Neil: it is situated 3 or 400 yards N. W. from the beautiful villa called Seaport on Port Ballinstay, a mile and a half west from the Giant's Causeway.

The face of the precipice here seems about 50 feet high, composed of horizontal strata of coarse basalt or trap, abounding with zeolite, and of a reddish tinge, friable, and decomposing; all these strata, from the summit to the sea, are cut through obliquely at an angle of about 45 degrees, by a dyke of sound blue basalt, very fine at its edges, but coarser in the middle, and nearly five feet thick: the fine basalt of this dyke and the coarse trapp of the strata, notwithstanding the difference of their grain, unite solidly on both sides of the dyke: this important fact is more easily ascertained here, than in any other dyke I know, it is so accessible. I must observe, that this dyke is not accurately rectilineal.

 direction,

direction, is lost under the rubble that has fallen from above; but whenever the precipice becomes perpendicular, it appears again in its last direction, cutting the strata vertically from the bottom of the precipice to the top, above 200 feet; the height of the upper part of the cliff above the sea is here 330 feet.

These strata are almost all columnar, and the horizontal prisms of the dyke are strongly contrasted with the vertical pillars of the strata.

The basalt of this dyke is very nearly of the same grain with that of the dyke at the Causeway, rather coarser, its fracture granular, and full of shining points; but it differs materially from it in another respect, the latter having but one principle of construction, to wit, the minute prisms into which it breaks, and the agglutination of these forming it into a mere wall; while the dyke at Port Spagna has, like some other varieties of our basalt, a double principle of construction, being first formed into huge massive prisms four and five feet in diameter, and these again being divided into small quadrangular prisms whose sides do not exceed an inch.

This property possessed by some varieties of our basalt, and other curious circumstances attending them,—as for instance, that some of our prismatic basalt in thin strata abound with marine exuviæ, shells, and impressions of cornua ammonis*, while others, columnar and prismatic, but not articulated, and others columnar, prismatic, and articulated, contain cavities full of fresh water to the amount of a thimble-full: all these facts have hitherto escaped notice.

Naturalists, who visit our coast, rarely allow themselves time enough to examine any thing, and, while there, are occupied in looking for arguments to support the theory they patronize, not in studying nature for information: they never examine any of our basalts but that of the Giant's Causeway; this, it is true, has none of the properties I mention, it has but one principle of construction, to wit,

* The nature of this stone is, I know, not yet fully ascertained. Sir Joseph Banks informs me, that the specimens I sent to him are pronounced by his friends not to be *genuine* or *legitimate* basalt. An eminent Scotch naturalist, who visited the spot last summer, I am told, asserts this stone to be *chort petrosilex* or *schistus*.

On the other side, Mr. Kirwan, to whom I gave specimens, asserts in a late publication, that it is basalt: our ingenious Mr. Higgins is of the same opinion, and the celebrated Professor Pictet of Geneva, who did me the honour of a visit last summer, considers it to be basalt containing a greater portion of *silex* than usual. I believe Mons. Pictet is right.

the

the visible prismatic form so much admired; this afterwards breaks indifferently in all directions.

To return to my subject:—Though the basalt septs in mines in general, and every one of our own whynn dykes at Ballycastle, are attended by a depression of the strata on one side; yet those I have described at Bengore Head are accompanied by nothing similar, except the one at the Causeway; and proceeding further eastward, coasting this promontory, we meet with three depressions of our strata, where nothing like a whynn dyke is to be found.

The first is singular and beautiful; it is near a mile east from the Causeway, and a quarter of a mile beyond the last dyke. The precipice here is uncommonly magnificent, its height more than 350 feet above the water; and the upper part of this, which is accurately perpendicular and extends half a mile on either side, is 150 feet.

This whole face is composed of three strata, two of them formed of superb basalt pillars 45 and 55 feet long, with an intermediate stratum, near 60 feet, of another variety of basalt; the lowermost of these strata, when produced westward, dips, and at its intersection with the sea forms the Giant's Causeway.

This grand façade, together with the whole promontory, is as it were cut down and bisected by a vertical plane, on the west side of which the promontory and all its strata have sunk and subsided about 40 feet, without any other shake or disturbance, all the strata in the subsided part still remaining accurately parallel to the permanent strata, and proceeding westward in their former direction, only from points 40 feet lower.

An account of the variety, arrangement, and alternations of these strata, so completely displayed in the superb face of this precipice, where nature seems to have intended to exhibit to the philosopher the order in which she has disposed her materials, without putting him to the trouble of penetrating into the bowels of the earth, would lead me far beyond the limits of a letter. How these strata, with their ascent, culminations, dip, and immersions, have hitherto escaped the observation of naturalists is quite beyond my comprehension*.

The two depressions further east are much inferior to this; I shall only observe that there is not the least appearance

* I cannot avoid quoting a passage from a late traveller, who seems to possess two qualities very necessary in a naturalist, to wit, *attentive observation* and *freedom from system*. He says, "No subject is more interesting or useful

pearance of crack or disruption, the strata on both sides of the depression are all consolidated into one mass.

When searching for whynn dykes upon our northern coast, I was obliged to omit about four miles of it lying between Bengore-Head and Carrickarede, as being too distant from Portrush and Ballycastle, where I was used to take boat, and totally void of shelter, even for the smallest craft.

To the westward of Ballycastle I saw only one dyke. On the east side of Kenbaan Point, a rock emerges from the water, which I have no doubt is part of a dyke, from the appearance it made; and as I approached it, I perceived it was formed of horizontal prisms: here too a new feature occurred, common indeed in the dykes further eastward, but which I had not observed in any of those I had yet examined; the centre and sides of this one were constructed differently, the prisms in the centre being larger than those in the sides, and all very neat, the grain too probably, as in other cases, also differed; but I was precluded from examining any of the circumstances which attended this curious little solitary rock, by the violent surf which then broke upon it.

Hitherto the precipice cut through by the whynn dykes, and the rocks from among which they sometimes emerge, were all basalt, uniformly stratified; but the accumulation of these strata, after regularly dipping, immerges beneath the sea to the westward of Ballycastle, and a new system of materials arises at the end of the strand to the eastward, to wit, alternate strata of freestone and coal; these are cut through exactly in the same manner the basalt strata were, by vertical whynn dykes, which all run into the sea, across the beach at the foot of the precipice.

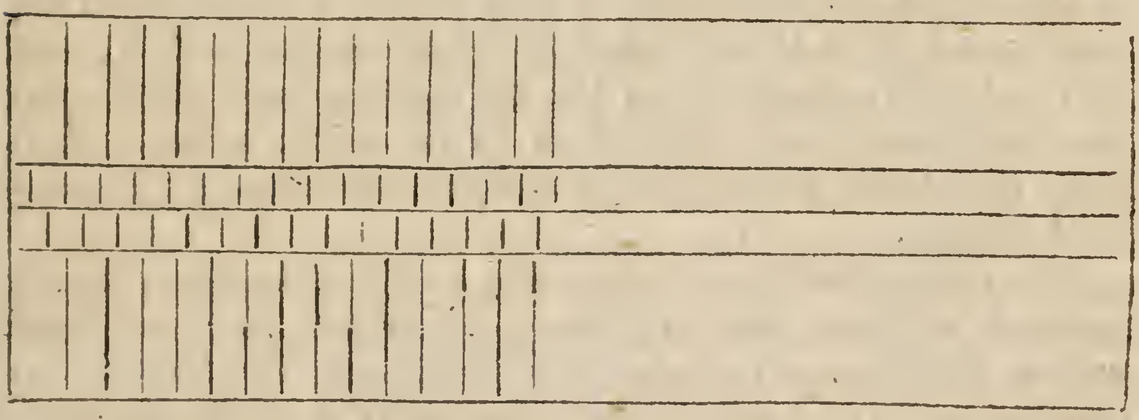
The first of these is about two miles from Ballycastle, and though a rude imperfect one, it is not to be overlooked; the black or dark blue of the basalt being strongly contrasted with the brown colour of the freestone it passes through on the beach; here the high road runs close under the precipice, and affords a good opportunity to examine the contacts of the basalt dyke with the freestone it cuts through.

The next dyke, some hundred yards further east, is more perfect,

useful than an examination of the intestine position of strata and veins:—in short, upon this is founded all our knowledge of geology; it is, however, attended with great labour and difficulty." (Jameson's Mineralogy of Scotland and Arran, page 61.) With us such an examination is attended neither with labour nor difficulty.

perfect, and so accessible on the beach, that its singular construction can be examined without any trouble; it is of the same breadth with most of the others, that is, about twelve feet; it more accurately resembles a quay than any of them, its surface is flat and its sides perpendicular, it is divided in its whole length by three right lines, one bisecting it through its middle, and one on each side of this, about a foot distant.

These three lines determine the style of masonry (if I may use the expression) with which it is built, to wit, horizontal prisms about five feet long, laid in rows on each side, and in the middle two rows of prisms about one foot square each. I attempt a skecth of these lines thus:



The bases of the long prisms show their polygonal figures on the sides of the dyke, and, if taken up and laid horizontally, would exhibit a rude *pavé:* these prisms are obviously composed of smaller ones like those at Port Spagna, but I had not a sledge sufficiently weighty to ascertain the fact with precision.

When I was on the spot, Mr. Magawly, who is concerned in and superintends the colliery, told me they were then cutting across this dyke 700 yards within the precipice.

The next dyke is of ruder basalt, and more imperfect; it seems to exhibit nothing remarkable.

The fourth Ballycastle dyke, or as it is called there the Great Gaw, emerges from beneath the precipice, of the same breadth and of the same rude material and construction with the first and third; but it is soon joined by what the colliers call its wing, that is a new wall annexed to it on each side, by which it becomes triple; these wings are of a very different material from the centre, being precisely the same in grain with the very fine Portrush stone, which sometimes contains shells and impressions of cornua ammonis, but in these wings I did not observe any.

* When this dyke enters the water, it accumulates into an island, or rock, of much greater height and breadth, still the two materials keeping distinct, though so united at the contact as to form but one stone: thus the arrangement of the coarse and very fine basalt here and at Portrush, is precisely the same, saving only one difference, that at the latter place the planes of the strata are horizontal, while at the Great Gaw of Fairhead they are vertical, and in both places grow into each other without interrupting the continuity or solidity of the material, yet leaving the line of demarcation distinct.

Though the precipice at this part of Fairhead be not so accurately perpendicular as at Bengore, yet the depression of the strata on one side of this dyke is visible from the water; and what is curious, a range of massive pillars, near 100 feet each, appears over the permanent part, while over the depressed part nothing is to be seen; whence it is plain that these strata have not been depressed by incumbent weight.

The miners tell me there is also a fifth dyke here, faintly marked without the precipice, while the gaw, or sept, within the mine is to them very important, and has also its depression on one side, like all the others at Fairhead, while at Bengore head no depression is found but in the dyke at the Causeway; all these depressions, as well as those at Bengore, where no dyke is found, are on the west side of the line, or plane, separating the permanent from the subsided part. I mention this curious fact for the information of geologists who may possibly make some use of it.

These singular walls are not confined to the northern coast of our basalt country; its eastern side abounds with them still more. It was not in my power to examine any of those except such as lie in the bay of Belfast, but my ingenious friend Dr. M'Donald (a zealous mineralogist, whose pursuits in that line have of late been much impeded by great success in his profession,) informs me that they commence near Murlogh, where my tour on that side ended; that they are very numerous about Torr point, Garron point, and in general on all projecting points on that coast; and he conceives (I think judiciously) that points being found where the dykes are most numerous,

* I mentioned before that some naturalists have denied this Portrush stone to be basalt; but its being found here in a whynn dyke seems strongly to support the affimative, as I have never heard of a whynn dyke composed of any material but basa . alone.

arises

arises from the protection they give the land in those places, preventing the sea from making the same inroads there it did on the adjacent parts.

Dr. M'Donald and I examined together the dykes at White-house point, four miles from Belfast; several of them are crowded together, three or four run parallel in an E. S. E. direction at about 150 yards from each other, and are in one place crossed by another at acute angles; several of these dykes, I am told, are traced across the county of Down on the opposite side of Belfast lough.

Though these dykes were so near, yet they differed materially from each other; in many the middle part and the sides were not of the same grain, nor constituted on the same principle; in some we found zeolite in the centre, but not in the sides; in others the middle part was formed by cutting it across (no doubt into prisms), while the sides were a rude mass studded with coarse round stones, about the size of an eighteen-pound ball; these last Dr. M'Donald assured me he had often broken, and found them composed of concentric spheres, like the pellicles of an onion; some of the dykes were of solid massive prisms laid quite across, while one or two had a longitudinal division running through their middle, as in the second dyke at Fairhead.

In all, the lines marking the construction of the dykes, whether accurate or faint, were across at right angles to their directions, but the perfection of the workmanship was very different; and when we attacked them with a light sledge, we found some to crumble, being in a state of decomposition, others resisted our efforts, while some broke into small quadrangular prisms, like the dykes at Port Spagna and the Giant's Causeway.

Dr. M'Donald showed me in his cabinet prisms he had taken from a quarry (no doubt a dyke) near Belfast; they were nine or ten inches long, and entirely composed of triangular pyramids of the same length, put together as if to illustrate Prop. 7. lib. 12th Eucl. Elem. I had found two or three small triangular pyramids among the quadrangular prisms at the Giant's Causeway dyke, but at the Belfast dyke triangular pyramids were the sole elementary figure.

As the shore in Belfast lough is low, there are but few opportunities of examining the materials that come in contact with the basalt dykes; in fact I noticed but two, stratified clay and freestone; this clay is very plentiful on the shore and the adjacent country; it is arranged in very thin

 horizontal

horizontal strata, and when exposed to the air hardens almost to the consistence of stone.

At the contact the basalt and freestone were strongly united together, and for two or three inches the basalt had in some sort acquired the colour and grain of the sandstone: I was particularly attentive to this fact, as Mr. Werner alleges the transition of basalt into other stones, and Mr. Jameson found in Arran (pages 131 and 135) basalt sometimes mixed with, and at others penetrated by, sandstone; but on this occasion Dr. M'Donald, by some experiments, found that notwithstanding the freestone appearance the stone remained pure basalt.

The basaltic area, from the north and east sides of which these singular walls diverge in such abundance, comprehends a considerable part of the country of Derry, and a much greater of the county of Antrim; its breadth varies from 20 to near 30 miles, and its length exceeds 35; it seems composed almost exclusively of vast and steady basalt strata accumulated upon each other; in one place we count 16, in others we conjecture more, especially at Magilligan rock, as we know the basalt to be 1200 feet deep there. This whole mass rests upon a vast stratum of white limestone about 200 feet thick, of the same extent with the basaltic area, but discoverable only at its periphery, which extends above 80 miles.

This mighty stratum ascends to the southwad, until its lower edge acquires on the east side a height of 800 feet, and on the west at least 1700; the country below the limestone stratum, and without it, is on the west side mostly schistus, on the east sandstone and clay penetrated by basalt dykes, which furnish stones in abundance for all purposes.

The Scotch whynn dykes have been generally supposed to originate in Ireland. If this fact be admitted, we can easily trace them by attending to the directions of our own: thus those that issue from the coast west of Ballycastle, proceeding north with a slight inclination to the east, are to be sought for in Islay, Jura, Mull, &c., where Mr. Mills actually found them in great numbers.

Our dykes which are seen at Murlog, Torr, and Cushendun, are obviously those which, having crossed the Mull of Cantyre, were observed by Mr. Jameson in such abundance in the Isle of Arran.

Dr. Hutton also mentions 20 or 30 whynn dykes he found "in the shire of Ayr to the north of Irvine on the coast." These correspond with the numerous dykes about

Garron

Garron point and its neighbourhood, whose rectilineal course is directed towards that part of the Scotch coast.

The dykes about Larne may be expected to be found on the Mull of Galloway, while those I examined far up in Belfast lough, on account of their S. E. direction, probably do not catch Scotland, nor meet land until they arrive on the coast of Cumberland.

Whether our whynn dykes be *identically* the same with those on the Scotch coast opposite, is not easily ascertained, though highly probable; but even confining ourselves to our own country, we find sufficient matter for astonishment in contemplating our basaltic area, formed by accumulations of horizontal strata, with numberless vertical planes radiating from it: had Dr. Beddoes been acquainted with this structure of our basaltic country, I think he would scarcely have asserted, that "a right knowledge of basaltes is conducting us fast to a just theory of the earth." I think very differently from Dr. Beddoes, and conceive that instead of *assisting*, basaltic facts are throwing new difficulties in the way of cosmogonists, who flatter themselves they have developed the secret of nature; and that those in my country, (to which I confine myself) are utterly irreconcileable to any theory I have met with.

Two sects of naturalists, distinguished by the names of *Volcanists* and *Plutonists*, have of late taken possession of all the basalt in the world, and have divided it between themselves, under the descriptions of *erupted* and *unerupted lava;* and they have so convinced Dr. Beddoes of the validity of their claim, that he says, "I shall assume the origin of basaltes from subterranean fusion to be thoroughly established."

After such a round assumption it may be deemed uncivil to question the igneous origin of our basalt dykes; but natural history is not to be sacrificed out of respect to confident assertion: I will therefore try by the test of *facts* whether that description of basaltes (which your lordship wishes for information upon) ever was in fusion.

Foreigners seem to know little of whynn dykes except in mines. Mr. St. Fond found at Chamarelle in Vivarois what is obviously a whynn dyke, and it embarrassed him more than any fact he ever met with; it will be found entertaining to look into his *Vol. ex. de Vivarois*, and into his *Min. des Vol.* to see the difficulties into which this *courant de lave compacte*, this *ruisseau de basalte en fusion* has thrown him, and the swingeing postulates he is obliged to make, in order to get over them.

Dr. Hamil-

Dr. Hamilton on behalf of the *Volcanists*, and Dr. Hutton the great advocate for the *Plutonic* system, are more ready at their expedients; the first of these forms our whynn dykes by pouring in erupted lava at the upper aperture of mighty chasms; while Dr. Hutton conceives these chasms were filled up by his own unerupted lava, forced up at the lower.

In discussing the opinions of these gentlemen, I will make them the most liberal concessions; for instance, I will concede to both, that they have discovered the process by which nature has formed chasms of immeasurable length, immeasurable depth, and of inconsiderable, though uniform, breadth.

I will concede to Dr. Hamilton that he has brought to the edge of the chasms his lava, "this foreign substance, which issuing from the vast mass of basaltes that forms the northern extremity of Fairhead, has descended over the adjoining strata," and that he has it ready "*to fill up each cleft and vacuity*." (Ham. Antrim, let. 5, part 1.)

I will also admit in favour of Dr. Hutton, that he has his unerupted lava ready at the bottom of these chasms, that he has his machinery prepared for forcing it up, and that he has surmounted his great difficulty, and discovered a mode of supporting such a mass when raised; a point upon which, having failed himself, he would discourage others from forming conjectures. (Edinburgh Trans. vol. i. page 285.)

Notwithstanding these concessions, it will not be difficult to show that these gentlemen have not discovered the secret of nature in the construction of these singular walls, and that they were not formed by liquid lava filling up mighty chasms.

1st. Many of our contiguous dykes differ materially from each other, yet their proximity is such, that, according to the theory of either Dr. Hamilton or Dr. Hutton, they must have been filled up from the same source, and with the same material.

2dly. Many of these dykes, both in Ireland and Scotland, show a material difference between their middle parts and their sides, both in grain and internal principle of construction; the change too is not gradual, but per saltum, as if the dissimilar parts were separated from each other by planes parallel to their sides: all this is perfectly incompatible with the high state of fluidity in which the lava must have been, to enable it to fill up vast chasms of such diminutive breadth.

3dly.

3dly. Our whynn dykes come in contact with a great variety of different substances, without producing such effect upon any one of them as might be expected from the contiguity of so glowing a mass; but however this argument may bear against the *Volcanists*, the *Plutonists* will say it does not apply to them, for the chemical operations of nature are carried on in Dr. Hutton's subterranean laboratory very differently from what we see on the surface of our globe: in the former Dr. Hutton says *calcareous strata are consolidated by the operation of heat and simple fusion*, and again, *having proved that these strata had been consolidated by simple fusion*, (page 253.) Dr. Hutton however confesses it is not easy to comprehend this: "and to be convinced that this calcareous stone, which calcines so easily in our fires, should have been brought into fusion by subterraneous heat without suffering calcination, must require a chain of reasoning *which every one is not able to comprehend.*" (page 271.)

But it is not necessary on this occasion to enter into the mysteries of a laboratory, to which we have not access, nor to calculate the force of Dr. Hutton's great agent *compression;* for our observations on the contacts of the matter of our whynn dykes with the substances they encounter, being made on the surface of the earth, in the open air, even admitting those dykes to be formed as Dr. Hutton supposes, his unerupted lava is now become erupted, and of course, to use his own words, "those substances which calcine and vitrify in our fires, should suffer similar changes when delivered from a compression which renders them fixed." (Edinburgh Trans. page 280.)

I am aware I must fatigue your lordship by dwelling so long upon the question of the igneous origin of our whynn dykes; but as most modern writers and travellers call them *lava veins*, and the *facts* I have observed with much attention, induce me to combat so general and so popular an opinion, I hope you will excuse me for adding a fourth argument, which I conceive to be conclusive.

All substances, when ignited, are in a high state of dilatation; this is followed, when they cool, by a contraction, *une retraite*, by which they occupy less space than they did when heated; of course, had our dykes been chasms filled up with glowing lava, when this material cooled and contracted, it could no longer fill up these chasms as before, but must crack and separate from their sides, leaving intervals and disruptions; but nothing like this is observed, the

the dyke and contiguous matter, whatever it be, are solidly united together, forming but one mass.

These whynn dykes suggest other curious questions: Were they formed at the same time with the contiguous materials?

Were they posterior to them, as Dr. Hamilton and Dr. Hutton suppose?

Or, were they antecedent to the stratified masses, that every where come in contact with them?

The inutility of such speculations deters me from entering into them. I must however confess, that the *facts* seem to give stronger negatives to the two first questions, than to the last. But who would hazard so bold an opinion, as that these mighty walls were the first part of our world that was formed. What an idea must it convey to us of this *frame-work* or *skeleton* of our globe!

A new theory I conceive more likely to be a nuisance than an acquisition to natural history; and that the road to the advancement of the science would be better laid open by destroying some of those we have already.

Should therefore your lordship think that the arguments I have adduced against the igneous origin of our whynn dykes are of any weight, I will probably make further inroads into the territories of *Vulcan*, and question the igneous origin of basalt in general.

To this your lordship will very likely reply, that the topic is worn threadbare; that most modern writers, without entering into the question, pronounce it to be already decided in the affimative; and that I shall never obtain attention to so stale a subject.

My opportunities, however, to procure information upon it have been superior to those of any other person; I have lived very many summers in the most important basaltic country in the world, and my fondness for the sea, and possession of boats, have enabled me repeatedly to explore our coast, which I know that no other naturalist ever did. It is to this coast and country that the advocates for particular opinions come to look for arguments to support the theories they patronize; it is painful to follow such gentlemen, correcting their statements, and contradicting their assertions: nor are they cursory travellers alone who misrepresent our facts; it will appear that men of science and ability are equally disposed to support their opinions at any expense;—a favourite theory is an adopted child, that must be maintained.

But

But it is not by exposing the errors of others that science, and especially *natural history*, is to be advanced; nor is it by puzzling ourselves to find out in what manner, and by what process, nature has executed her work; let us rather examine attentively what she has actually done; let us quit disputing about the whimsies of our own brains, and study the code of *facts*.

In our basaltic country these are curious, as well as abundant; and it will be from such of these alone as have escaped the attention of my predecessors, and from the geological construction of the country, that the arguments to be applied to the question of the igneous origin of basalt will be drawn; and whatever may be their weight, at least they will have the merit of novelty to recommend them.

I am, with great respect,
Your lordship's
Most obedient humble servant,

Portrush. W. Richardson.

P. S. When I found an opportunity for examining the whynn dykes to the northward of Whitehouse-point, I omitted several under the demesne called Macedon, which were much covered by sea-wrack; here I knew the surfaces of the dykes were decomposed, and their distinctive characters defaced.

Between Macedon and Carrickfergus there are many, all as usual differing from each other; some not so rectilineal in their course as those I have hitherto described; in one or two the prismatic construction was scarcely perceivable, while in the greater number the arrangement of these prisms laid across the dyke was most distinct.

In two contiguous dykes I observed, that the axes of these prisms were not horizontal as usual, but in one greatly elevated to the north, and in the other towards the south.

Human attention could not follow the variety which nature has displayed in the formation of these dykes; therefore, not to fatigue the reader, I will describe but two more particularly; I select these, both on account of the new circumstances attending them, and also because they are easy of access, being within a few yards of the great road from Belfast to Carrickfergus.

The first of these runs eastward along the strand, about 400 yards south of the gallery; we approached it from the north, and found it composed of long well-formed horizontal prisms, lined on the north side by a sort of basaltic wall about 18 inches thick: this a military gentleman of

our

our party called its *revetement;* I adopt the word on this occasion for convenience.

After we had traced the dyke eastward for several yards, we observed this revetement separate from it, and diverge at a considerable angle, then, forming a curve, disappear beneath the sand to the north-east; this new circumstance exciting our attention, we traced the revetement back to the dyke, then along it to the westward, when after some time we perceived it entering the dyke at an acute angle, and crossing it diagonally; when across, it formed for several yards a *revetement* on the south side of the dyke, then diverging from it, and curving as before, it was again lost under the sand to the south-west.

The second dyke I will describe particularly, lies about 500 yards north from the *silver stream*, and about three miles from Carrickfergus; it seemed composed of four or five distinct walls, agglutinated together; in each of these the prismatic construction was different from that of the others, and in one the axes of its prisms were not as usual at right angles, but oblique to the direction of the dyke.

A new circumstance occurred here too; this dyke, about 25 feet broad, had a *revetement* of freestone on each side, and was also twice or thrice penetrated by walls of freestone similar to, and in the same direction with, the basalt walls between which they lay; these freestone walls were more than a foot broad, and sometimes composed of horizontal laminæ, and at others of vertical.

I have since discovered a magnificent dyke in the face of the stupendous precipice of Cave hill* which it cuts vertically near 200 feet, and is afterwards to be traced a great way down the hill.

Though this dyke be attended by very curious circumstances, I will take no further notice of it, as I hope to see it soon accurately described by my ingenious friend Dr. M'Donald, who was with me when I discovered it, and whose vicinity affords him better opportunities of accurately examining this beautiful and interesting façade.

LVIII. *Notice respecting New Books.*

A NEW edition of Dr. Henry's chemical work is in the press, and will be published in the course of a few weeks.

* A stratified basaltic mountain, nearly hanging over Belfast; it is well worth the attention of naturalists.

He

He has found it impossible to give a sufficiently ample and distinct view of the numerous and important discoveries which have been made in the science during the two last years, without extending the work to a second volume. And as its original title would but ill accord with the enlarged form under which the book will now appear, it will be changed to that of "ELEMENTS OF EXPERIMENTAL CHEMISTRY."

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On the 3d, 10th, and 17th of May, the reading of Mr. Macartney's observations on luminous insects was continued. The result of the author's researches has enabled him to state that 12 different genera of insects, which include an immense number of species, emit light. Seven of these genera belong to the order of *mollusca*, and the other five to the hemipterous, as the *fulgoræ;* the apterous, as the *cancer pulex* and *fulgens*, &c. Mr. Macartney took an historical survey of what has been written on luminous insects, related the discoveries of different voyagers, such as those of Sir Joseph Banks, who discovered two species, the *cancer pulex* near Madeira, in 1774, with capt. Cook, and the *medusa noctiluca.* Capt. Horsburgh also discovered two species in the Arabian sea, which he gave to the author, one of them like a wood louse, the other he called *medusa scintillans.* The same accurate observer also noticed various luminous appearances of the sea, and some insects, which on being pressed emit a luminous fluid. He also gave to Mr. M. a drawing of one of the insects, which he took out of the water at a time when the sea appeared almost white, like a vast field of ice covered with snow. This appearance is ascribed by the author to immense quantities of *medusa scintillans*, which emit flashes of light, and so frequently as to assume a continued brightness. To this species of *medusa* the author attributes the sudden flashes of light which are occasionally seen on our own sea shores. Mr. M. has also discovered three different species of luminous insects on the southern coasts of England. In the course of his inquiries, he appears to doubt the luminousness of the *cancer pulex*, but notices the *pyrosoma atalantica*, a worm-shaped luminous insect,

insect, observed by M. Perot, of which only one genus or species has yet been discovered.

In summing up some concluding remarks on the cause of this luminous quality in insects, Mr. M. expresses himself with great diffidence, and from the experiments of Rumford, and his own observations, hesitates in stating whether light is not rather a quality than a substance, as all the phænomena of luminous insects tend to give probability to the former opinion. The *medusa*, he observes, can emit light for any indefinite time: their light and that of other sea-insects appear of no specific use to the animal, but that of glow-worms and flies serves to make them known to each other in the night. All luminous animals shun the light of day, and hence the author infers that they cannot have imbibed solar light sufficient to emit so much during the night; that the luminous matter of the sea, or *medusa*, has nothing in it phosphoric or inflammable; that the manner of secreting this luminous matter (if so it be) is yet wholly unknown; that the sudden death of the animals, and consequent extinction of their luminousness; prevent all anatomical or microscopical observations, and that the number of creatures possessing this peculiarity is very considerable. He related a great number of experiments, all of which tended to demonstrate that this light has nothing in it of a phosphorescent quality, as universally believed, and that, whatever it may be, it is no longer attributable to the presence of phosphoric or inflammable matter.

May 24. The introduction to a paper on the sexual organs and mode of generation of the *squalus* genus, or dog-fish, by Mr. Home, was read. It related principally to a description of those organs in the fishes, of which Mr. H. has before given some general accounts to the Royal Society.

LINNÆAN SOCIETY.

April 17.—The President in the chair. The following papers were read: Observations by Olof Swartz, M.D. on some former Species of Andromeda, properly belonging to the Genus Menziesia:—On the supposed Effects of Ivy on Trees, by Humphry Repton, Esq.:—On the Fasciola Hepatica, by Mrs. Cobbold.

May 1.—A paper was read On the Genus Andræa, with Descriptions of four British Species, by W. J. Hooker, Esq., F.L.S.

Thursday, May 24, being the anniversary of the birthday of Linnæus, the Linnæan Society met at their house in

in Gerard Street, in pursuance of their laws and charter, to elect a president, council, and officers for the ensuing year: when the following five new members of council were elected:—John Blackburne, esq. Edward Forster, esq. George Milne, esq. Edward Rudge, esq. and Edward Lord Stanley. And the following were chosen as officers: James Edward Smith, M.D. president; Thomas Marsham, esq. treasurer; Alexander MacLeay, esq. secretary; and Mr. Richard Taylor, under secretary.

The society afterwards dined together at Freemasons' Tavern, as usual.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this society, on the 7th April, Dr. Macknight read a mineralogical notice, on the tract of the Highlands from Killin to Braemar, by the way of Glen Tilt. Ben Lawers is composed of undulated mica-slate, which at the summit is yellowish gray, and in some varieties so full of quartz as to resemble a sandstone. Towards Logierait, beautiful garnets begin to appear. Beyond Mullenearn, gneiss occurs; also limestone, hornblende-slate, and sienite. Besides the substances first mentioned, Glen Tilt is remarkable by a peculiar aggregate of feldspar, hornblende, and occasionally quartz; in which the various proportions of these ingredients exhibit the rock under various aspects of the sienitic and greenstone species. It is distinguished from granite (for which it has been mistaken) not only by the uncrystallized state of the feldspar, but by the presence of hornblende, and the absence of mica. Professor Jameson has entitled it sienitic greenstone. It occurs in conformable beds; particularly one of great size, which intersects the channel of the river at different places, near the lodge. Crossing the mountains from Glen Tilt to the course of the Dee, we find hornstone, feldspar-porphyry, and limestone, subordinate to mica-slate and gneiss; till we reach the Castletown of Braemar, where the granite of the Grampians at length appears.

At the same meeting, a communication from colonel Imrie was read, describing the conglomerate-rock of the Grampians, and tracing it from near Stonehaven to the Burn, and again at Callender, 80 miles distant. The position of this conglomerate-rock is nearly vertical; and of this fact, in col. Imrie's opinion, no satisfactory explanation has yet been given.—At this meeting, also, there was laid before the society an accurate section of the coal-field at Alloa, accompanied with interesting remarks, by Mr.

Robert Bald, civil engineer, and manager of Mr. Erskine of Mar's extensive coal-works. The depth of the section is 704 feet; the alternating strata are 141 in number; and the total amount of the different beds of coal is 59 feet 4 inches.—Captain Laskey likewise presented to the society a series of the remains of a curious fossil Encrinus found in slate-clay near Dunbar.

FRENCH NATIONAL INSTITUTE.

[Continued from p. 317.]

GEOLOGY.

The observations from which geology can derive the most important advantages, are certainly those which are directed to the subjects of fossil animals, but more particularly those which lived upon dry land. M. Cuvier has continued the investigation of this important subject. He has brought to a termination, in conjunction with M. Brongniart, the mineralogical geography of the environs of Paris, a slight sketch of which was given in the account of the labours of the Class for the year 1808 *. He afterwards directed his attention to the osseous heaps (*brèches*) on the shores of the Mediterranean. Rocks similar to those which are to be seen at Gibraltar, near Terruel in Arragon, at Cette, at Antibes, at Nice, in Corsica, in Dalmatia, and in the isle of Cerigo, have been found in the fissures of the compact limestone which constitutes the principal soil of these various places, and they are all composed of the same elements: it is a red cement, like brick, which connects in a confused manner numerous fragments of bones and of limestone in which these heaps are contained. All the bones found in these rocks belong to herbivorous animals, most of which are known still to exist on the adjoining soil; they are mixed with fresh-water shells. This inclines us to think that these heaps are posterior to the last continuance of the sea on our continents, but very ancient nevertheless; since nothing proves that similar heaps have been recently formed, and some of them, such as those in Corsica, contain even unknown animals.

Alluvial earths also contain bones of herbivorous animals: they have been discovered in the peat-mosses of the valley of the Somme, with stags' horns and heads of oxen, and in the environs of Azoph, near the Black Sea. These bones have belonged to a species of beaver: the former resemble those of the common beaver; the others, which

* See page 36 of the present volume of the Phil. Mag.

form

form a complete head, seem to have belonged to a much larger species than we are acquainted with; and M. Fischer, who discovered this animal, has given it the name of trogontherium, which M. Cuvier adopts as the specific name.

Bones of herbivorous animals have also been found in schists. Three kinds have been described. M. Cuvier saw the figure of one which some authors regarded as having appertained to an Indian boar, and others to a polecat. M. Cuvier rather gives it the character of a herbivorous animal; but he has not been able to ascertain the genus nor species.

Among the fossil bones of ruminating animals found in loose strata, M. Cuvier has recognised a kind of elk different from that with which we are now acquainted. The bones of this animal have been found in England and Ireland, near the Rhine, and in the environs of Paris, in beds of marl at no great depth, and they seem to have been deposited in fresh water. Some horns discovered in abundance in the neighbourhood of Etampes, in sand surmounted by limestone of fresh water formation, prove the existence of a small species of rein-deer which seems no longer to exist. M. Cuvier has besides observed remains of horns of goats, fallow-deer and stags, which do not seem to differ essentially from the horns of the existing species: "Nothing," says our author, "is more abundant: all the recent alluviations dug up have furnished them; and if we do not find plenty of testimony as to these fossil bones, it is because from presenting themselves at trifling depths they have not been thought worthy of much notice."

In the fossils of ruminating animals with hollow horns, M. Cuvier has recognised crania of aurochs, discovered in the banks of the Rhine and the Vistula, in the environs of Cracow, in Holland, and in North America. These crania exceed in size those of the aurochs; but, as M. Cuvier observes, this difference may be ascribed to the abundance of food which these animals formerly possessed, when ranging at pleasure through the vast forests and pasturages of France and Germany.

There is another kind of fossil cranium differing only from our present oxen from the size, being larger and the horns being in a different direction. These crania have been found in the valley of the Somme, in Suabia, Prussia, England, and Italy. "If we recollect," says M. Cuvier, "that the ancients distinguished in Gaul and Germany two kinds of wild oxen, the urus and the bison; may we not suppose that one of the two, after furnishing our present

race of oxen, was extirpated in his savage state; while the other, which could not be subdued, still subsists in small numbers in the forests of Lithuania alone?"

In loose soils we also meet with bones of horses and of wild boars: the former almost always accompany the fossil elephants, and are found along with the mastodonti, tigers, hyenas, and other bones of fossil animals discovered in alluvial soils: but it was impossible to ascertain if these horses' bones belonged to a species different from our present race. The bones of wild boars have been for the most part procured from peat-mosses, and do not in the least differ from those of the wild boars of the present day.

Other bones have been found, which M. Cuvier has ascertained to belong to an unknown species of *lamantin* or *manati*. They have been discovered in strata of coarse marine limestone on the banks of the Layon, in the environs of Angers; and they were mixed with other bones, some of which seemed to have belonged to a large species of *phocas*, and the others to a dolphin.

The skeletons of three species of oviparous quadrupeds, preserved in calcareous schists, have also been the object of M. Cuvier's researches.

The first was found in the schists of Œnigen, situated on the right bank of the Rhine, at the mouth of the lake of Constance. It had been described and engraved as the skeleton of an antediluvian man; but this error was refuted. M. Cuvier proves by a series of osteological inquiries that this reptile was analogous to a salamander, and belongs to the genus *proteus*.

The second, also found in the same place, seems to have belonged to the toad genus, and resembles the *bufo calamita*.

The third, and most singular, which was discovered in the quarries of Altmuhl, near Aichtedt and Pappenheim, in Franconia, and which had been described and drawn by Collini in the Memoirs of the Manheim Academy, is regarded by M. Cuvier as having belonged to a species of otter. The length of its neck and head, its long snout armed with sharp teeth, and its long paws, indicate that this animal fed on insects, and that it caught them when flying: the size of its orbitary sockets also shows that it must have had very large eyes, and that it was a nocturnal animal, like the bat. No beast of the present day has the least resemblance to it.

M. Cuvier, has also published a Supplement to his Memoirs on the Fossils of Montmartre; in which he gives the figure and description of an ornitholite, much more complete than those which have been hitherto published. It is probable

probable that it belonged to the class of *gallinacei*, and the common quail is the modern species which it resembles.

M. Sage has given us the description of some carpolites, or petrified fruits. One of them was the kernel of a nut become calcareous, and found at Lous-le-Saulnier: another seemed to have been the fruit of a wild nutmeg-tree, which grew at Madagascar and in some of the Moluccas; its substance was also calcareous: the third seemed to have belonged to something resembling the durion of India; it was transformed into jasper. To these new facts he subjoins some of the remarks which had been already made on carpolites, and concludes that the petrified fruits found in our climate are exotics. He also enters into some chemical details, by means of which he explains how these petrifactions took place.

BOTANY.

Order and method will always be two objects of the first importance in natural history, and particularly in botany: they serve at one and the same time to establish the relations which bodies have with each other, and to guide the observer in the midst of the innumerable productions of nature. The most celebrated naturalists have made it the particular object of their studies; and the knowledge which the real science of the various systems requires, could never have been embraced but for them.

M. Jussieu, who has so just a title to be considered as the legislator of botany, has formed a new order of plants under the name of Monimiæ: the genera of which it is composed are, the *ruizia*, the *monimia*, the *ambora*, and perhaps the *citrosma*, the *pavonia*, and the *atherosperma*. This order ought to be placed immediately before the family of the Utriceæ; but after the Monimiæ M. Jussieu places the *calycanthus*, heretofore united to the Rosaceæ; he considers it as the type of a new order, which will serve as a stage between the Monimiæ and the Utriceæ.

M. Palisot Beauvois has proceeded with his inquiries into the order of Gramineæ. He has studied their organs of fructification more exactly than any person had done before him; has founded on the organization of each of the parts of these organs the characters which ought to distinguish them from each other, and obtained the means of dividing the different species of this order into genera, much more natural than those which had been hitherto adopted.

M. Labillardiere has made us acquainted with a new plant

plant of the family of Palm-trees, of which he has made a genus under the name of *ptychosperma*, and placed it next the elati and arecas. This plant was discovered by the author in New Ireland: it rises frequently to the height of 60 feet and upwards, and yet its trunk is only two or three inches in diameter. These proportions induced him to give it the name of *gracilis*. It is astonishing, as M. Labillardiere remarks, that so slender a tree can support itself; but we know that in all the monocotyledons, the hardest of the ligneous part is external, and this structure gives to the plants of this class a strength which those cannot possess whose most solid fibres are in the centre.

M. Lamouroux has presented to the Class a very extensive work on marine plants. Little or no attention has been paid to these singular vegetables, and they have been arranged in rather an awkward manner: M. Lamouroux, by forming into a single group all the plants known to exist in the sea, seems to have wrought an advantageous change.

The little progress which had been made in the study of the algæ, was the cause of the disagreement among botanists as to the organs which serve to the reproduction of these Cryptogamia. M. Correa, in a work written expressly on this subject, had recognised male and female organs in the tubercles placed at the extremities of the ramifications of these plants. M. Lamouroux partakes of this opinion; but he characterizes with precision the different parts of these organs, and thus throws a great deal of clearness on the study of these singular vegetables. This author has besides observed that the kind of algæ which grow on granite, are never the same with those found on calcareous stone or on sand, and *vice versâ*. As to their internal organization, M. Decandolle had ascertained that it was devoid of vessels, and entirely formed of cellular texture. M. Lamouroux distinguishes two kinds of cellules; the one being long hexagons, which form the stalks and the nervous parts (*nervures*) of the ramifications; the other kind is of the same form with the foregoing, but has sides almost equal, and which constitute the membranous or foliaceous substance.

M. Lamouroux thinks that the former may be analogous to the vessels, and the second to the utricular texture of the most perfect vegetables. These general labours led the author to form in this family several new genera, which he has also presented to the Class for their approval.

M. Mirbel has continued his researches on vegetable physiology. Formerly it had been ascertained that the albumen of

of the seed generally served for the nourishment of the young plant after germination: but this opinion perhaps had still need of support from positive observations; and M. Mirbel, by means of an experiment equally simple as ingenious, seems to have dispelled all doubts on the subject. The embryo contained in the grain of the albium cepa becomes curved on being developed, so as to form a tail which issues from the ground, while the radicle and plumule still remain under it. If at this period of vegetation we make any mark at an equal height on the two branches of the germ, we shall see the speck nearest the radicle rise alone in the case where the plant receives no nutriment except from the juices of the earth: if, on the contrary, it be only kept up by the albumen of the seed, the speck of the plumule will rise above the other: lastly, the specks will rise nearly equally, if the earth and the seed concur to the development of the germ. It is this last phænomenon that takes place; it ceases when the albumen is entirely absorbed: in that case the young plant has sufficient strength to derive from the earth, or from the atmosphere, the nutriments which it will immediately require.

This memoir is accompanied by interesting observations on the germination of the asparagus, and on the manner in which the leaves of this plant, sheathing themselves at first like all those of the monocotyledons, become by the growth of the stalk, lateral and opposite, and afterwards lateral and alternate.

In another memoir M. Mirbel has undertaken some new enquiries respecting the germination of the nelumbo. Botanists were not agreed as to the class to which this plant ought to be referred, and as to the nature of the two fleshy lobes in the midst of which it takes its origin. Some, not observing any radicles developed in the germination of this plant, thought that it was entirely devoid of them; some regarded the lobes just mentioned as roots; and others regarded them as peculiar organs, and analogous to the vitellus. It is by means of anatomical observations that M. Mirbel endeavours to dispel the doubts which these various opinions have raised. He recognised in the first place, in the nelumbo, all the characters which distinguish the plants with several cotyledons from the plants with a single cotyledon. He afterwards found in the lobes of this plant vessels analogous to those of the cotyledons, and he observed, at the point where these lobes join, other vessels which are united in the same manner with those which characterize the radicles in the embryos furnished with this organ:

 and

and he concludes that the nelumbo does not differ essentially from the other plants of its class.

M. Correa, although he agrees with M. Mirbel that the nelumbo is a plant with two cotyledons, does not share in his opinion respecting the nature of the lobes: he thinks, with Gærtner, that these organs have a considerable analogy with the vitellus, and he compares them with the fleshy tubercles of the roots of the orchis. The plants, as this learned botanist observes, have a double and relative organization.—on the one hand, with the earth in which they ought to take root, and on the other hand, with the air in which their foliage is developed; the roots as allotted to the ascending vegetation, and the leaves to the descending vegetation; and it is at the point where these two systems of organization unite, that the cotyledons are generally placed:—Now the lobes of the nelumbo are at the most inferior part of the plant, and consequently in the system of the ascending vegetation, or of the roots. This view of regarding the nelumbo would indeed take away the means of recognising the cotyledons in it; but the example of many other plants deprived of these organs, shows that they are not at all essential to vegetation, and that the characters which have been derived from them, in order to separate the vegetable kingdom into three divisions, are insufficient, and that they ought to be replaced by those which give the direction of the vessels and the medullary radii.

It is also with the view of dispelling the doubts arising from the different opinions of several learned botanists, that M. Poiteau has undertaken a work, which he has submitted to the Class, on the germination of the Gramineæ. Botanists were not agreed as to the part of the seed of these plants which ought to be regarded as the cotyledon; but observing that the escutcheon, which Gærtner took for a vitellus, and M. Richard for the body of the radicle, was placed in the point where the plumule and the radicle separate, he considers this organ as a true cotyledon. These inquiries have besides led M. Poiteau to an observation, which, although accidental, is not the less interesting, since it is connected with one of the phænomena which are most general in vegetation. At the moment when the radicle of the Gramineæ is developed, it takes the figure of a cone, and represents the principal root or the pivot of the other plants; but soon afterwards, and the instant the lateral roots acquire a certain growth, this cone is obliterated and destroyed, so that no plant of this family has a pivot: and as

as M. Poiteau has made the same observation on several other plants with a single cotyledon, we may suppose that this substitution of numerous roots and secondary to a principal root takes place, because each fasciculus of fibres of the monocotyledonous plants has its proper root. This naturally recals to our minds the fine observation of M. du Petit-Thouars on the increase in size of the dracæna, which has been discussed in the reports of preceding years.

ZOOLOGY.

The researches of M. Cuvier respecting fossil animals have generally led him to preliminary discussions as to the species admitted by naturalists, and they have been almost always the source of some valuable observations in zoology properly so called. Thus in his Memoir on the osteology of the lamantin, when considering the organization of the amphibious mammiferæ, he is led to separate *phoci* and *morsi*, the *dugóus*, the lamantins and the species described by Steller which had been confounded with these last animals. These three genera form a family which is distinguished among its members by the absence of the posterior extremities, and by the teeth of herbivorous animals: he reduces to two the four species of lamantins established by Buffon, and gives precise characters to those which he admits into these different genera.

In another Memoir, on cats, the same author gives the osteological character of the head of the chief species of this genus, and he gives an account of one which had not been recognised by modern naturalists. This new species has received the name of leopard, which had become synonymous with panther, for want of a precise application. It differs from this last species in being of a smaller size, and having more numerous spots.

M. Geoffroy had long formed under the name of Atèles a particular division of apes devoid of thumbs, which had been formerly confounded with the sapajons, from the catching tail which is common to all these animals. He has added two new species to those which he had already given an account of, and has given figures and descriptions of them. One of these only, to which he gives the name of Arachnoides, and which is yellow, had been described by Edwards and Brown. The other denominated *Encadrée* is entirely new; it is black with white hair around the face.

The same author has given a description of two birds, the one scarcely known, and the other entirely new: this last

last has a resemblance to the *corvus nudus*, and to the *corvus calvus;* but they differ sufficiently to form three distinct genera, which M. Geoffroy establishes by the names of *cephalopterus*, being his new species, *gymnoderus*, which he applies to the *corvus nudus*, and *gymnocephalus*, by which he distinguishes the *corvus calvus*.

The cephalopterus is black, with a very high crest, which falls forwards upon the beak, and a kind of dewlap, also covered with feathers. The feathers of both these parts are of a metallic violet hue.

The second bird, which like the above is also from Mexico, had been described but imperfectly by Marcgrave under the name of cariama. M. Geoffroy from this description had considered it as closely connected with the agami; but now that it is to be seen in the collection of our Museum of Natural History he regards it as forming a distinct genus, to which he gives the Latin name of *microdactylus*.

Tortoises have also been one of the subjects of M. Geoffroy's researches. Having observed in Egypt the tortoise of the Nile described by Forskahl, he was induced to form a distinct genus, of all the other tortoises which like the latter have the extremity of the sides at liberty and a soft calipash. He calls them *trionix,* and has added several new species to these already known. M. Brongniart in his great work on reptiles had joined the latter to his *Emydes*, observing always the characters which distinguished them from the other species of this genus of which the calipash is complete and covered with scales. M. Geoffroy, in addition, joins to the genus *Chelys* of M. Dumeril, the tortoise described by Bartram under the name of tortoise with large soft scales, and discovered by this traveller in North America.

These animals present a striking example of the progress of zoology of late years. The number of tortoises known 20 years ago was scarcely 30, and now it has been at least doubled. This among other circumstances has been communicated to us by a work of M. Sweiger, in which he has undertaken to give a general monography of all the tortoises. This fine work, accompanied by precise descriptions of a very extensive synonymy and embellished with figures excellently drawn by M. Oppel, has been submitted to the inspection of the Institute, and highly approved of.

The class of fishes has also been enriched with many new species. Messrs. Risseau and Delaroche, who have particularly directed their attention to this branch of zoology, have communicated their observations to us. Those of

of the former have been made on fishes in the Gulf of Nice, and those of the latter were made on the fishes in the sea around the Balearic Islands. But the labours of these naturalists have not been confined to bringing new species to light:—from their accounts there are grounds for supposing that each species of fish, like terrestrial animals, has a region, in the midst of which its existence is circumscribed, and that those of the south are never met with in the north, and *vice versâ*. M. Risseau, however, has discovered in the Mediterranean some fishes which had not hitherto been found except in the Indian or in the Northern seas.

M. Delaroche has made some interesting researches as to the depth at which each species of fish lives habitually, as to the modes of fishing, and on the subject of the swimming-bladder.

PHYSIOLOGY.

Physiological experiments of all others are those which require most leisure and patience, while the rigorous exactitude so important and necessary in the sciences is more difficult of attainment in physiology than in any other branch of experimental philosophy. Humboldt, however, while occupied on a voyage in which obstacles and dangers were daily increasing, directed his attention to some delicate experiments on several of the phænomena of life. He has communicated the researches which he made in America on the respiration of the crocodile with the sharp beak: he was led to ascertain "that this animal, notwithstanding the volume of its bronchiæ and the structure of its pulmonary celluli, suffers in an air which is not renewed; that its respiration is very slow:—in the space of an hour and 43 minutes, a young crocodile, three decimetres in length, took up only about 20 cubic centiemes of oxygen from the surrounding atmosphere."

Since his return to France, M. Humboldt, in conjunction with M. Provençal, has made some additional inquiries into the respiration of fishes. The experiments of these gentlemen, which are numerous, and remarkable for their accuracy, have led them to very important results.

The experiments of Spallanzani, and of our colleague, had demonstrated that it is not by decomposing water that fishes breathe, as some naturalists thought; but by taking up the oxygen dissolved in this liquid, or by coming to the surface of the water they collect it immediately from the atmosphere. To these observations all our knowledge on the

the subject was confined: the nature and quantity of the gases had not been established, which were absorbed by these animals in the act of respiration; and the result of these phænomena, the chief object of the experiments of Humboldt and Provençal, is to elucidate these questions. With this view they first consider fishes in their natural state, and respiring river waters; they next examine the action of the bronchiæ on the ambient water impregnated with oxygen and azote, carbonic acid, or a mixture of hydrogen and oxygen, and they afterwards treat of the changes produced by fishes on different aëriform fluids in which they are plunged.

Seven tench (*ciprinus tinca*) were placed under a bell-glass filled with river water, and which contained 4000 cubic centimetres: after eight hours and a half respiration, the fishes were withdrawn, and the analysis of the air still remaining in the water showed that in this space of time the fishes had absorbed 145·4 of oxygen, 57·6 of azote; and that 132 of carbonic acid had been produced. Hence it results, as observed by our authors, "that by the respiration of the fishes submitted to this experiment, the volume of the oxygen absorbed exceeded only by two-thirds the volume of the azote which had disappeared, and that no more than one-eighth of the former had been converted into carbonic acid."

Fishes suffer greatly in water entirely freed of air; and after 20 minutes they fall motionless to the bottom of the vessels. In pure oxygen these animals seem to respire with avidity, and open their bronchiæ more widely. In azote and hydrogen, they keep their bronchiæ close, seem to dread the contact of these gases, and die very soon after having been plunged into the water which contains them. Carbonic acid kills them in a few minutes: but fishes do not absorb oxygen and azote by their bronchiæ alone: the whole surface of their bodies has the faculty of acting on these gases, and of assimilating them. After having withdrawn the fishes from water saturated with the deleterious gases, and analysed it, some portion of carbonic acid was found in the liquid; but as there had been no oxygen absorbed, it is probable, as observed by Messrs. Humboldt and Provençal, that it was not the result of respiration, but that it had been exhaled from the surface of the body. Such are the principal points in this work, which besides contains other useful observations and interesting views on the physiology of fishes, but which the limits of this report do not permit us to enter upon.

In

In speaking of respiration, however, we cannot pass over in silence a memoir read to the Class by M. Provençal, on the respiration of mammiferæ in which the nerves of the eighth pair had been cut. We have already spoken of the experiments which had been made to ascertain the influence of these nerves on respiration, by which this influence was demonstrated: some doubts, however, remained as to the way in which it is exercised. M. Provençal was anxious to ascertain if the animal in which the eighth pair of nerves had been cut, absorbed as much oxygen, and produced the same quantity of carbonic acid, before, as after the operation. Numerous experiments made with care proved that the animal, after the section of the nerves, absorbed less oxygen and produced less carbonic acid than before this section; but these changes are produced by gradation only. At first the respiration does not appear weakened; subsequently, it is effected with less strength; and finally these phænomena cease entirely, but most probably in consequence of the cessation of the mechanical functions of the chest. It was interesting to ascertain if the animal heat diminished in the same proportions with respiration. M. Provençal therefore made all the experiments necessary for resolving this question; and it would seem that in fact the temperature diminishes soon after the nerves have been cut and the respiration has slackened.

The functions of the organs whose action has been just mentioned, are well known; but there exists in animals a certain number of other organs whose functions are not evident, and as to the use of which physiologists are still divided in opinion. Among this number is the swimming-bladder of fishes. This singular organ, which is only to be found in this class of animals, is not met with in all the species; and it exhibits so many varieties in its organization, that at the first glance one would incline to think that it did not perform the same functions in some that it did in others. Generally this bladder is filled with air, and composed of two membranes. Sometimes it communicates with the stomach by a canal: at other times it has no apparent communication, and in this case it contains a peculiar organ of a red colour and of a lamellated structure, according to the observations of M. Duvernoy. There are some bladders, however, which are furnished with these red bodies, and which have a canal of communication; and some, but few in number, have peculiar muscles. The opinions of authors vary as to the use of this organ and of its different parts: in general it has been thought that it was employed

employed to change the specific gravity of fishes, and that for this purpose the animal, by means of its muscles, compressed this organ, and thus varied its dimensions according as it was desirous to remain in equilibrium, to ascend or descend in the medium in which it exists. As to the way in which the air gets admission, it has been thought that it was by means of the canal in such bladders as are furnished with it, and by means of the glands by secretion in those which have no communication externally. Besides, we know from the experiments of M. Biot, that this air is a mixture of oxygen and azote, and that its nature varies according to the depth at which the fish lives: so that the species which are procured from the bottom of the sea contain a greater proportion of oxygen, while those which are found near the surface yield more azote. M. Delaroche having collected a great number of fishes in the Mediterranean, has described their swimming-bladders, and made us acquainted with some which were unknown before: he has confirmed the experiments of M. Biot; and as to the uses of this organ, he adopts nearly the same results with preceding naturalists.

This air-bladder has also been the subject of some researches by Messrs. Humboldt and Provençal. They wished to ascertain what were the relations between this organ and respiration. The chief results of their experiments are as follow:—The air contained in the swimming-bladder does not depend upon the air brought into contact with the bronchiæ; the absence of this organ does not affect the process of respiration, but it seems to prevent the production of the carbonic acid gas. Lastly, they have seen tench, from which the swimming-bladder had been taken, swim about, rise and sink in water, with as much facility as those which are furnished with them.

These labours have given rise to a very detailed report of M. Cuvier, in which he brings to view all the inquiries which have been undertaken as to the swimming-bladder of fishes, and in which he again takes notice of the various questions which have arisen on the subject. After a profound discussion, he arrives at some general results which we have mentioned above, and details every thing which still remains doubtful on the subject*.

There are some other experiments from which physiologists may derive great advantage. These relate to the action exercised by substances of various kinds on the bodies of

* See the present volume, p. 291—302.

animals

animals when introduced into the circulation. Medicine, indeed, affords many experiments of this description: but they are still too few in comparison with those which might have been tried.

[To be continued.]

LX. *Intelligence.*

DE LUC'S ELECTRIC COLUMN.

In our last number (p. 317), by a typographical omission we stated that "since that time (viz. 25th March) they (the bells) have been known once to cease ringing."—The sentence should have read "since that time they have *not* been known once to cease ringing."

We have since received notice, that on the 21st of May the small bells connected with De Luc's electric column were still ringing, and it was supposed had continued so without intermission since the 25th of March.

We are also desired to request that those of our readers who may have constructed columns of the kind alluded to, will favour us with such observations upon them as they may make from time to time.

LIST OF PATENTS FOR NEW INVENTIONS.

To Luke Hopkinson, of Holborn, in the county of Middlesex, coach and harness maker, for certain improvements to a bridle bit, or bits for bridles used in driving or riding horses or other animals. May 2, 1810.

To Daniel Beaumont Payne, of the city of Bath, banker, for his new plan or method for more accurately and expeditiously expressing and ascertaining the number, dates, and sums in bank bills, notes, and other securities for money, and preventing forgeries, frauds, and losses, by defacing or altering the same. May 2.

To William Clerk, esq. advocate, for his method for preventing smoke, dust, and the danger of fire, and for increasing and regulating heat from stoves and chimney fireplaces for heating rooms, halls, passages, and stair-cases in public-buildings and dwelling-houses, and all other apartments where regulated heat and cleanliness are desirable, without obstructing the view of the burning fuel. May 2.

To Sebastian Erard, of Great Marlborough-Street, in the county of Middlesex, for certain improvements on piano-fortes and harp. May 2.

To John Maiben, of Perth, in the county of Perth, sadler and ironmonger, for certain apparatus for making carbonated hydrogen gas from pit-coal, and for using the same for lighting mills, factories, houses, lamps, &c. the lights being regulated by means of syphons. May 2.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For May 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	44	58°	47°	30·16	67	Fair
28	47	64	48	·14	74	Fair
29	48	69	52	·05	76	Fair
30	51	68	54	29·98	86	Fair
May 1	49	67	44	·85	68	Fair
2	44	52	46	·80	45	Cloudy
3	44	49	45	·70	29	Cloudy
4	45	47	40	·76	26	Cloudy
5	40	51	36	·80	36	Fair
6	40	50	40	·80	35	Fair
7	41	48	46	·55	0	Rain
8	49	62	50	·67	51	Fair
9	52	61	50	·80	40	Cloudy
10	52	65	51	·98	42	Cloudy
11	47	55	42	30·08	43	Fair
12	42	56	45	29·97	41	Cloudy
13	50	47	47	·72	22	Cloudy
14	49	59	49	·60	51	Fair
15	49	55	50	·35	15	Cloudy
16	48	61	49	·45	25	Fair
17	49	54	52	·46	0	Rain
18	40	43	44	·56	0	Rain
19	42	59	44	·98	41	Fair
20	45	60	48	·95	47	Fair
21	57	61	52	·61	40	Fair
22	54	60	49	·87	37	Showery
23	50	60	48	30·10	42	Fair
24	49	61	47	·13	76	Fair
25	47	60	49	·05	46	Fair
26	46	68	47	29·93	61	Fair

N.B. The Barometer's height is taken at one o'clock.

ERRATUM.

Page 303, of the present volume—Article Royal Society, Mr. Groombridge's formula—for " tang. $\overline{2-3{\cdot}36\,r}$" read " tang. $\overline{Z-3{\cdot}36\,r}$"—Z representing Zenith distance.

LXI. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen. By* HUMPHRY DAVY, *Esq. Sec. R.S. F.R.S. E. M.R.I.A.**

I. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries on the application of Electricity to Chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

II. *Some new Experiments on the Metals from the fixed Alkalies.*

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phænomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phænomena, different from those I adopted.

MM. Gay Lussac and Thenard, as I have mentioned

* From Philosophical Transactions for 1810, Part I.

on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hydrogen; a similar opinion seems to be entertained by M. Ritter. M. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hydrogen, with the alkalies; and an Inquirer† in our own country regards them as composed of *oxygen* and *hydrogen*.

I shall examine such of those notions only as have been connected with experiments, and I shall not occupy the time of the society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which MM. Gay Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur; and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mem. d'Arcueil, a copy of which M. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium‡ in ammonia, and they found that a considerable quantity of ammonia was absorbed; and hydrogen produced; and that the potassium became converted into an olive-coloured fusible substance; by heating this substance strongly, they obtained three-fifths of the ammonia again, two-fifths as ammonia, one-fifth as hydrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two-fifths, and found in the vessel in which the operation was carried on, nothing but potash.—Again, it is stated, that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm that they

* *Journal de Physique*, June 1808.
† Nicholson's Journal, August 1809, p. 258.
‡ *Mem. d'Arcueil*, tom. ii. page 309.

can

can procure from a given quantity of ammonia, more than its volume of hydrogen.

Whence, they ask, can the hydrogen proceed?—Shall it be admitted that it is from the ammonia? But this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. Berthollet, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogen gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogen, and that alkali.

It is obvious, that even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water.* And of the oxygen which this would give to the potassium, and of the hydrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shown, by numerous experiments, many of which have been repeated before members of this society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hydrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions [Phil. Mag. vol. xxxiv. p. 344.] and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the

 potassium

potassium was employed in quantities of from three to four grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases there was a considerable deficiency of hydrogen, as well as nitrogen, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases I obtained more than four-fifths of the potassium which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at 30·2$^{in.}$, thermometer at 54° Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7·5 were absorbed, and 3·2 of hydrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors, was exhausted, and filled with hydrogen. They contained together $\frac{8}{10}$ of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxygen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed, was such as to generate by its action upon water three cubical inches and $\frac{3}{10}$ of hydrogen gas.

Now if this experiment be calculated upon, it will be found, that 7·5 — ·2 = to 7·3 of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hydrogen. But the 3·2 cubical inches of hydrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process, = 9; and the nitrogen in the 8·8 cubical inches of gas (or the 9 — ·2 of ammonia) will be about 3, and if we estimate ·34 of hydrogen, and ·16 of nitrogen, in the ·5, remaining in the retort; there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon

upon the $\frac{8}{10}$ of hydrogen pre-existing in the tube and adaptors, the loss of hydrogen will be found proportionally rather greater than that of nitrogen.

In another experiment in which three grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air; 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors, the same quantity of residual air, as in the process last described.

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia; and the unabsorbable part detonated with oxygen, in the proportion of 11 to 6, gave a residuum of 7·5.—The barometer was at 30·2$^{\text{in.}}$, thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6·78—·4 of ammonia=6·38, and this quantity of gas decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hydrogen; but there are produced in this experiment, of hydrogen, 2·48 in the first operation, and 4·28 in the second, and considering the nitrogen in the permanent gas as 3·32, ·8 must be subtracted for the common air; which would give 2·52 for the nitrogen generated; and to these must be added, the quantity of hydrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hydrogen.

In all experiments of this kind, a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort containing oxygen gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from three to four grains of potassium were made

to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from four to six grains of this powder formed; but I have advanced no further in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxygen.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture, and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hydrogen. When copper tubes are employed, the hydrogen bears a smaller proportion to the nitrogen; and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium or nitrogen; but a loss smaller or greater of hydrogen.

It will be asked, On what do these circumstances depend? Does the affinity of certain metals for potassium prevent it from gaining oxygen from ammonia, and do platina and copper combine with a small quantity of hydrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded but the volatile alkali, and that the *hydrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygen and hydrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

$3\cdot\frac{3}{10}$ grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hydrogen, and the fusible substance, which was very similar to that from potassium, distilled,

distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and, by detonation with oxygen, proved to consist of nearly two of hydrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phænomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal, which is decompounded in this process.

As sodium does not act so violently upon oxygen, as potassium; and as soda does not absorb water from the atmosphere, with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form, evidently, by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies are composed of the *alkalies* merely united to *charcoal;* the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which MM. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in eight cubical inches of oxygen: nearly two cubical inches of oxygen were absorbed, and soda in a state of extreme dryness, so that it could not be liquefied by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the

phænomena which I have described in the Bakerian lecture for 1807. Nearly six cubical inches of hydrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination; and it appears from M. Curaudau's method of decomposing the alkalies, that his metals must have been carburets not of potash and soda, but of potassium and sodium.

M. Ritter's argument, in favour of potassium and sodium being compounds of hydrogen, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygen than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogen; yet though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly*, yet sodium is heavier than potassium in the proportion of 9 to 7 at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygen than sodium; and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxygen is given off at the positive surface, no hydrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogen than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on

* *Hassenfratz, Annal. de Chim.* tome xxviii. p. 11.

potassium,

potassium, and I find that instead of proving that potassium is a compound of potash and hydrogen, they confirm the idea of its being as yet like other metals undecompounded.

When tellurium is made the positive surface in water, oxygen is given off; when it is made the negative surface, the Voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate fiom it, and diffuse itself through the water; the water gradually becomes opake and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogen in water; which, in being diffused, is acted upon by the oxygen of the common air, dissolved in the water, and gradually loses a part of its hydrogen, and becomes a solid hydruret of tellurium. The compound of hydrogen and tellurium produced at the negative pole when uncombined is gaseous at common temperatures, and when muriatic acid or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large Voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which when touched by water, did not inflame nor effervesce, but rendered the water of a beautiful purple colour, and when thrown into water entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *telluretted* hydrogen (if the name may be used) had formed with the oxidated potassium, *i.e.* the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogen; metallic tellurium was formed where it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced

produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was further confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogen was evolved by the action of the alloy upon water; but the compound of telluretted hydrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hatchett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aëriform* compound of tellurium and hydrogen; I shall mention merely some of its most remarkable qualities, and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hydrogen is more analogous to sulphuretted hydrogen, than to any other body. The smell of the two

two substances is almost precisely the same*. Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic, but which is soon converted into muriate of tellurium †.

As arsenic has an affinity for hydrogen, it occurred to me as probable, that it would present some phænomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark-coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the nega-

* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, esq. of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr. Warburton co-operated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of Voltaic electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets, at the Voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogen in the Voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and further researches, made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

† From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogen, enters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

tive

tive surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogen by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic* were heated together in hydrogen gas, they combined with such violence as to produce the phænomena of inflammation, and an alloy was produced of the same kind as that formed by means of the Voltaic battery.

As tellurium and arsenic both combine with hydrogen, it appeared to me probable, that by the action of alloys of potassium, with tellurium and arsenic, upon ammonia, some new phænomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendour, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogen, instead of being pure hydrogen, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas which was principally nitrogen; so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hydrogen; which are contradictory assumptions.

None of the chemists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in

* In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy Connais Chem. tom viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that though potash is decompounded in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogen. The gases not absorbable by water given off in this operation, are peculiar. Their smell is intensely fœtid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogen: whether they are mixtures of various gases, or a single compound, I am not at present able to decide.

which

which agent, which they did not suspect were concerned. No person, I believe, has attempted to show that when potassium or sodium is burnt in oxygen gas, water is formed, or that water is generated when potassium decomposes any of the acids *; and no one has been able to form potassium, by combining hydrogen with potash. I stated in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxygen gas, *the pure* alkalies were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxygen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed; yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made upon considerable portions of potassium and sodium, procured by chemical decomposition.

When potassium is burnt in trays of platina, in oxygen gas that has been dried by ignited potash, the absorption of oxygen is about $\frac{1}{2}\frac{1}{0}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain †. The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shown by some very well conducted inquiries, that potash and soda ‡, in their common state, contain a considerable proportion of water; and M. Ber-

* When in October 1807, I obtained a dark-coloured combustible substance from boracic acid, at the negative pole in the Voltaic circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. MM. Gay Lussac and Thenard have likewise operated upon boracic acid, by potassium, and they conclude that they have decompounded it; but this does not follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash: the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

† The quantities of gas given out by the operation of water, are in a similar ratio. See page 43 of the last Bakerian lecture [Phil. Mag. vol. xxxiii. p. 432,] and page 26 of this paper [p. 407 and 408 preceding.]

‡ *Annales de Chimie*, Nov. 1808, page 175.

thollet

thollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in the 100 parts 66·66 potash, and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from ten parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner: five grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light*; and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about five of hydrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

The second experiment was conducted with still more attention to minuteness. Eight grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phænomena as in the last experiment, and the increase of weight of the tray was 6½ grains. The muriate of potash was kept for some minutes in fusion in the tray,

* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

till

till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxygen, to form 9·4 grains of potash, and 6·6 — 1·4 = 5·2, the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash; but 35·6 of muriatic acid, according to M. Berthollet's estimation, would demand 71·1 of alkali, in the state of dryness in which it exists in muriate of potash, and 71·1 — 64·4 = 6·7 — so that the potash taken as a standard by M. Berthollet, contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali *.

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

Thess conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides: this part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

[To be continued.]

* Consequently M. Berthollet's fused potash must contain nearly 28 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.

LXII. *Ob-*

LXII. *Observations respecting a New Scale for the Thermometer.* By RICHARD WALKER, *Esq. of Oxford.*

THERE are four different thermometers in use at this time, viz. Fahrenheit's, Reaumur's, Celsius's, and De l'Isle's.

Fahrenheit's scale, which is used in Great Britain, has the *zero*, or commencement of the scale, placed at 32 degrees below the freezing point, and 212 is the boiling point of water.

Reaumur's, or the French scale, modified by De Luc, in which the 0 is placed at the freezing point, and from thence to the boiling point are 80 degrees*.

Celsius's scale is used in Sweden; in this 0 is placed at the freezing point, and from thence to the boiling point are 100 degrees.

De l'Isle's scale is used in Russia; in which 0 is placed at the boiling point of water, and the freezing point is 150.

With respect to Fahrenheit's scale, it may be considered, now, as having no foundation whatever in any principle, and is in fact upon that account universally disapproved of, and evidently upon the decline.

Reaumur's scale, and others in which the freezing point of water has been adopted for placing the 0, or commencement of the scale, has been hitherto deemed the least objectionable, on account of its being an invariably fixed point.

In the construction of thermometers two fixed points are required: accordingly, the scales of all thermometers have hitherto been, and probably ever will be, adjusted by means of the freezing and boiling points of water; the latter, as is well known, being an equally fixed point with the former, under certain circumstances well known to the philosopher and the artist.

The freezing and boiling points of water, then, may be considered as applicable only to the due arrangement of the proportions and precision of a thermometrical scale, and consequently either of *them* unfit for the place of commencement of it: hence the place where 0 should be placed is yet a desideratum.

Considering the thermometer as a measure of heat, leaving the negative term *cold* out of the question, the pro-

* A thermometer called the centigrade has lately been introduced in France, which is in fact no other than that of Celsius, with the addition of decimal divisions.

per

per place for 0 would undoubtedly be that point at which heat commences; but the physical causes, which are known to exist, to prevent that point being ever ascertained, exclude entirely the hope of fixing it there; to say nothing of the inconvenience which must arise from the scale being incumbered with a multiplicity of figures, at that part which is most familiar and in most frequent use.

Hence we are under the necessity here, as in many other instances, of using the positive and negative signs.

Having been for a very long time engaged in thermometrical experiments and observations,—the imperfection of all the scales in use, and the consequent dissatisfaction of various persons respecting them, frequently occurred to me; and it is long since that I was impressed with the opinion, strengthened by that of several of my friends, of the preference which was due to the one I am now about to mention.

It has been ascertained by physiologists as a fact, and of which I have perfectly satisfied myself, by repeated experiments on others as well as myself, that 62 of Fahrenheit is that point at which the human body in a state of health *is unconscious of either heat or cold*, that is, in a state of rest, or when free from any considerable bodily exertion; and this is really the case at all seasons of the year, in this climate, and probably in all other climates; for the temperature of the human body, or blood heat, as it is called, is determined every where to be 98, and it has been found that the vital functions have the power of regulating the sensation of heat; so that any temperature *above* 62 of Fahrenheit, under ordinary circumstances, shall give a sensation of *heat*, and any temperature *below* 62 of Fahrenheit a sensation of cold *.

* Dr. Cullen, in speaking of the influence of temperature on the human body in this climate, says: "If the temperature at any time applied is under 62 degrees, every increase of temperature applied will give a sensation of heat; but if the increase of temperature does not arise to 62 degrees, the sensation produced will not continue long, but be soon changed to a sensation of cold. In like manner, any temperature applied to the human body lower than that of the body itself, gives a sensation of cold; but if the temperature applied does not go below 62 degrees, the sensation of cold will not continue long, but be soon changed to a sensation of heat." Cullen's Practice of Physic, vol. i. 1784, p. 91, 92.

This point is so nicely balanced, and so accurately just, viz. at 36 degrees below blood-heat, or 98, that a variation of one, or at most two degrees, above or below that point, actually produces a sensation of heat or cold; and which, by experiment properly made, would be found to be equally the case in the torrid and frigid zone as in temperate climates: hence this point may be considered as the actual, or natural *nought*, with respect to heat and cold on the thermometer.

Upon this principle the present scale is founded, which I cannot help thinking is perfectly unexceptionable, being fixed on an unalterable basis; and that in a thermometer thus constructed, there will be ever a coincidence in the central point of this thermometer with that precise temperature which is most congenial to the feelings of the human body, and prove universally and permanently a correct standard for reference, and consequently, I should think, render this instrument more intelligible and interesting, and of course more extensively useful.

With respect to the divisions, I adopted those of Fahrenheit from an opinion of that being the fittest, considering those of Reaumur, the centigrade, &c. as being too few, and decimal divisions unnecessary in a thermometrical scale.

Hence it will follow, that 0 being placed at 62° of Fahrenheit, 150° will be the boiling, and minus 30° the freezing, points of water; and all other points on Fahrenheit's scale may be reduced to this, by subtracting 62 for any degree *above* 0 of Fahrenheit's, and adding 62 for any degree *below* 0.

This thermometer may be considered as particularly appropriate to the purpose of regulating the heat of rooms, &c. and showing the exact variations in the air from that temperature which is at all times most congenial and salutary to the human body.

The difficulties that may occur in the introduction of a new thermometrical scale, are not more, perhaps less, than in many other innovations, viz. the New Style—the New Chemical Nomenclature, &c.

The mechanical method of graduating this thermometer is thus: the freezing and boiling points of water having been taken in the usual way, the freezing point is to be marked 30, and the boiling point 150; this space is then to be divided into six divisions, or parts =30° each, and the 0 placed at the first division above 30, or the freezing point; the scale is then to be completed by marking in the tenths above and below 0, continuing the graduation to any desired extent.

Sir Isaac Newton, who I believe was the person that originally fixed on the freezing and boiling points of water, *conjointly*, for the adjustment of a thermometrical scale, places his zero at the freezing point, from a notion of that being the utmost degree of cold; and Fahrenheit, for a similar reason, placed his zero afterwards at 32 degrees below the freezing point, the degree of cold which he

he had produced by a mixture of ice and salt, which he at that time believed to be the greatest degree of cold in nature*.

There is no scale which I have met with, which seems to have been founded upon the principle I have adopted: thermometers indeed have been constructed in England, for particular purposes, in which 0 was placed at what was supposed to be the *middle state of the air* in this climate; but this principle is of too local and vague a nature to merit attention.

I know of one objection, only, which can be started against the adoption of the alteration in the scale I have proposed, viz. the too frequent occasion, as might be urged, for the use of the plus and minus characters; but the fact is, that in a scale founded upon this principle, there is less occasion than ever for the use of either of them, as must be apparent to any one upon the least reflection.

For meteorological observations this scale will be particularly appropriate; the zero in this instance being the mean temperature of the greatest heat and greatest cold experienced in the hottest and coldest climates, *collectively*, as well as in the temperate climates†.

At Quito in Peru, which is peculiarly situated between the extremes of heat and cold, the temperature of the air is uniformly, or with very little variation, throughout the year at 62 of Fahrenheit; and this is considered to be the healthiest spot in the world‡.

If we had no means of correcting or regulating the temperature of our apartments by fire, we should find a few degrees, viz. ten *below* the point of 62, much more uncomfortable than the same difference *above* 62; and in a difference of thirty degrees from that point the cold would be intolerable, in the first instance; whereas at thirty *above*, no considerable inconvenience is experienced: but the difference would be exactly the inverse of each other, were it

* Sir Isaac Newton's thermometer was constructed in 1701, and Fahrenheit's in 1724.

† In some parts of Africa the thermometer rises sometimes up to 112 *above* 62 of Fahrenheit, viz. 174, and in some parts of North America it sometimes sinks to 112 *below* 62 of Fahrenheit, viz. −50. A similar circumstance takes place in the temperate or middle climates; thus in England the thermometer rises sometimes to 126; and sometimes sinks to −2, viz. a difference, each way, of 64 degrees.

‡ The mean annual temperature of Quito is 62; and the utmost limits of variation throughout the year are from three or four degrees *below* that point, to as many degrees *above* it.

not that the human body, from a well-known provision in the animal œconomy, has the innate power of resisting, or rather of diminishing, the effect of extreme heat to a certain extent.

My chief object in the present paper has been to inforce the propriety of the alteration suggested, in the place of the *zero* in a thermometrical scale. With respect to the fittest mode of division to be adopted, I have preferred that of Fahrenheit, for the reason above stated; but since the mode of decimal divisions seems at the present time to be gaining ground, I have exhibited a scale of the centigrade division likewise in the annexed table. (See Tab. II.)

It will be apparent, in adopting the centigrade division in the stead of Fahrenheit, that there will be a difference, occasionally, amounting to $\frac{1}{3}$d of a degree, between them; but as the *three leading points* correspond precisely, so slight a variation in the others is of little or no consequence; and indeed all the points cannot be perfectly accurate upon any uniform scale: thus, the precise point at which quicksilver freezes was ascertained to be -38 and $\frac{2}{3}$ of Fahrenheit, but is marked at -39.

There are four different proportions of the scale, which I should recommend to be used according to circumstances; viz. 1st, a portable scale extending from -30 to $+30$; 2dly, one for ordinary meteorological use in this climate, extending from -70 to $+70$*; 3dly, from -100 to $+150$†; and 4thly, a thermometer of *coloured alcohol*, extending from -162 to $+40$‡.

I flatter myself that I have adduced sufficiently satisfactory reasons for the adoption of the scale I have here suggested; and, when the usual prejudices for an old system have subsided, and the apparently insurmountable difficulties attending the introduction of a new one are conquered, that the one I have now offered to consideration will be admitted to be perfectly reconcileable in a philosophical view; convenient and useful in application; not liable to be affected by new discoveries or fresh theories in future; and, in short, a scale dictated as it were by nature for universal application.

* A local scale of this kind in every climate would be particularly useful in the place itself, and interesting every where else.

† The freezing point of quicksilver upon this scale is -101; but as quicksilver ceases to measure temperature at -100, the scale of a quicksilver thermometer may terminate there.

‡ Quicksilver is preferable to alcohol for thermometers, as far as its range goes, for several reasons; but, principally, because it is found to be more uniform in its expansion by heat than alcohol or spirit.

P. S.—Be-

P. S.—Before I became acquainted with the circumstance which induced me to adopt the scale which I have now proposed, I endeavoured, with a view of placing the zero at the lowest natural point, to obtain, by inference, the utmost degree of *natural cold.*

In order to effect this, I formed the following table, which is deduced from Mr. Kirwan's table of the *mean annual temperature* of every latitude; in which, of course, the latitudes within nine or ten degrees of the pole were deduced, like mine, by inference drawn from the others: and I presume, by comparing the results in this table with actual observations as far as these have gone, they will be found to accord sufficiently, to show that the principle I have adopted, in deducing the results, may be relied on. (See Tab. I.)

Hence it appears, if the results in this table are admitted to be correct, that the scale would commence at 68 degrees below the present 0 of Fahrenheit, or 100 degrees below the freezing point of water; and that any greater degree of cold than this, which already has or may hereafter be produced by art, would, *alone*, require the *minus* sign.

The greatest degree of artificial cold hitherto upon record is −91 of Fahrenheit: how far future experiments may carry this point, or whether there be a *finite point* attainable by art, is perhaps impossible to say.

Knowing that cold is produced by adding snow to *alcohol*, and presuming that alcohol is the last liquid in nature which would freeze, or whose power of producing cold with ice reaches to a lower temperature than any other substance, I formerly entertained a hope of ascertaining the *ultimate point* to which this could possibly be carried; but partly on account of the difficulties of such an undertaking, but more especially in consequence of other pressing avocations which deprive me of the opportunity, I have relinquished my intention, at least for the present.

The method I should have pursued would have been to have formed mixtures of alcohol and nitric acid, increasing the proportion of the alcohol as the temperatures at mixing became lower; finally, using alcohol alone with snow, till I should arrive at the points where alcohol itself ceased, when mixed with snow, to produce cold.

The attempts hitherto made for ascertaining what is usually called the *natural zero*, carry that point so extremely low, and the results are so very discordant with

each other, that any idea of fixing the 0 there, I should think, must be entirely relinquished.

With respest to the freezing point of mercury being, as has been suggested by several philosophical persons, a fit place for the zero, it perhaps may be sufficient to observe, that this would be adapting a scale of heat to a *particular thermometer*, instead of applying a thermometer to a scale of temperature.

The table of temperatures applies more particularly to the different degrees of latitude specified, *from the equator to the north pole in the eastern hemisphere*, where the gradations or diminutions of heat are tolerably uniform. In the western hemisphere, the diminutions of heat from the equator to the north pole are found, from causes well known, to be very irregular or anomalous; and in general the cold is considerably greater on the same parallels of latitude in this, than in the eastern hemisphere.

From the equator proceeding towards the *south pole*, the diminutions of heat are found to be tolerably uniform; the chief difference consisting in the cold being somewhat *greater* in the higher latitudes, on the same parallel, than on the north side of the equator.

Notwithstanding the irregularity as to diminution of temperature which is observed in the latitudes between the equator and the polar regions in the western hemisphere, and the difference before mentioned on the south of the equator; there is good reason to suppose that, *at the poles*, as at the equator, the variation of temperature is not considerable, because *the maximum of cold* (or minimum of heat) is *at the poles*, as the *maximum of heat* is at the equator*.

Hence, although every latitude between the equator and the poles may be liable to considerable anomalies or irregularities, with respect to heat and cold, according as they are more or less exposed, so as to receive the currents of air from higher or lower latitudes; yet at those two points where there is *only one of these causes* operating upon each, the variations in the temperature, at the same seasons, in different years, may be but small, comparatively with the others, particularly the middle latitudes.

In latitudes, therefore, even at some distance from the

* The maximum of cold at the poles is perhaps *more complete* than the maximum of heat, from a well-known cause, is at the equator: viz. the sun's annual path, respectively to the earth, being *not confined to the line of the equator.*

poles,

poles, (as in latitudes at some distance from the equator,) the temperature may be nearly or quite the same as at the poles: thus, at Albany Fort, Hudson's Bay, N. latitude 52, W. longitude 82, the thermometer has sunk to −50, which is the greatest degree of natural cold ever *observed;* and is within eighteen degrees of that which I have *estimated* to be the *greatest natural cold.*

TABLE I.

A Table of the Temperatures of different Latitudes.

Latitude.	Kirwan's Table.	Mean Temperature.	Greatest Cold.	Greatest Heat in Shade.	Greatest Heat in Sun.	Series of Ninety.	Series of Nine.	Series of Nine.	Series of Nine.
90	31·	31·	−64	52	78	95	21	26	4
80	32·6·	33·	−52	55	82	85	22	27	6
70	37·2·	37·	−38	60	88	75	23	28	8
60	44·3·	44·	−21	68	97	65	24	29	10
50	52·9·	53·	− 2	78	108	55	25	30	12
40	62·	62·	+17	88	119	45	26	31	12
30	70·7·	71·	+36	98	130	35	27	32	10
20	77·8·	78·	+53	106	139	25	28	33	8
10	82·3·	82·	+67	111	145	15	29	34	6
0	84·	84·	+79	114	149	5	30	35	4
A	B	C	D	E	F	*d*	*e*	*f*	*g*

Nota bene.—A is the latitude, in tenths of degrees, from the equator to the pole.

B is Kirwan's table of the *mean annual temperature* of each latitude in A: the heat increasing, *but in a diminishing ratio,* from the middle latitude, viz. 45, the *mean annual temperature* of which is 57·5 to the equator; and the cold increasing, *but in a diminishing ratio,* from latitude 45 to the pole.

C is Kirwan's table B reduced to the nearest whole numbers.

D is the *greatest cold* of each latitude A, deduced by subtracting the number in *d*, from the parallel number in C, up to latitude 40, and from thence up to latitude 90, *subtracting* the number in C from the number in *d;* the first producing the plus degrees, and the latter the minus degrees.

E is the *greatest heat in the shade* of each latitude A, deduced

deduced by adding the number in *e* to the parallel number in C.

F is the *greatest heat in the sun* of each latitude A, deduced by adding the number in *f* to each of the parallel numbers in E.

d is an *increasing* series of *ninety*, in number corresponding with the degrees of latitude from 0 to 90, in column A, viz. from 5 to 95.

e is a *decreasing* series of *nine*, in number from latitude 0 to 90, viz. from 30 to 21.

f is a *decreasing* series of *nine*, in number from latitude 0 to 90, viz. from 35 to 26.

g is a *decreasing* series of *nine* in number from the middle latitude of 45 up to 90, and the same down from latitude 45 to 0— latitude 45, being 13.

The temperatures marked in columns D, E, F, give the temperatures of *ordinary years;* in order to give the defect or excess of *extraordinary seasons* the parallel number in *g* must be *subtracted*, or *added* to either of the columns in D, E, F.

Mr. Kirwan has given the *mean annual temperature* of every latitude, from the equator to the north pole: in this table *each tenth* only is given, but the temperature of each intervening latitude may be obtained easily by calculation from those given, viz, by allowing one-tenth for each degree of latitude, in the columns *d*, *e*, *f*, *g*.

N. B. The thermometer, in taking observations, is supposed to be placed at about 5 or 6 feet from the ground, and perfectly insulated or detached from any body which can cause reflected heat, when placed in the sun; and for observations in the shade, placed in a north aspect, where the sun never reaches.

Hence, according to this calculation, the greatest cold at the poles, or the *greatest natural cold*, is at —68 of Fahrenheit, or 100 degrees below the freezing point of water; therefore, in order to reduce Fahrenheit's scale to this, 68 must be *added* to every degree of Fahrenheit's *above* the 0; and for any degree *below* the 0 of Fahrenheit, that number must be *subtracted* from 68.

It might be expected, as the gradations in the above table are uniform, and calculated from the equator to the poles, that the temperatures in this table should correspond with considerable exactness with actual observations in every instance; but it should be recollected, that a difference must occasionally arise from various causes, viz.

difference

difference of elevation; vicinity to, or distance from, the sea †; the intervention of mountainous and woody countries, intercepting the currents of hot or cold air, from lower or higher latitudes ‡; difference of soil, as to its power of absorbing, accumulating, or reflecting heat, &c. All of which irregularities, however, it might perhaps be possible to calculate and allow for.

From the equator to 23½ degrees on each side of it, the progressive variations in temperature differ somewhat from the other latitudes; this is allowed for in the columns of the mean annual temperatures, and likewise in the three succeeding columns, which may be considered as *emanations* from the first.

I shall now conclude by presenting the following Table, (*see next page*) which exhibits a comparative view of the scale of Fahrenheit's thermometer with mine.

Explanation of the four Columns.

Nota bene.—A is my proposed scale, having the zero at mean temperature.

B is Fahrenheit's scale.

C is the centigrade measure, adapted to column A.

D is my former scale, having the zero at the point I have estimated to be *the greatest natural cold.*

The four additional points marked thus * are appropriate to this climate, viz. the *greatest degree of cold,* which was observed on the morning of December 25, 1796: and the *two greatest degrees of heat*, viz. in the *shade* and in the *sun*, observed in the afternoon of July 13, 1808.—The *temperature of springs*, means of course the *constant* temperature of ordinary (unmediated) springs, which in all climates corresponds pretty exactly with the mean annual temperature of the place, which in London is 51·9.

N. B. Thermometers have a point marked *temperate*, viz. at 56 of Fahrenheit; but 57½ is the *middle point* of the scale, that being the mean annual temperature of latitude 45. RICHARD WALKER.

Oxford, May 31, 1810.

† Hence it follows that in islands, particularly *small islands*, surrounded by a large extent of sea, the winters are *warmer*, and the summers *colder*, than on continents.

‡ The winters in those parts of North America which have been cultivated, are much less rigorous now than formerly, principally in consequence of the destroying of forests, &c. which present several sources of cold, amongst which *evaporation* is no inconsiderable one.

TABLE

TABLE II.

A Table of the most essential Points in a Scale of Heat.

W.		F.	W.	W.
+150	Water Boils.	+212	+100	280
+114	Spirit Boils.	+176	+76	244
+64	* Greatest Solar Heat in England.	+126	+43	194
+50	Fever Heat.	+112	+33	180
+36	Blood Heat.	+98	+24	166
+30	* Greatest Shade Heat in England.	+92	+20	160
+14	Summer Heat.	+76	+9	144
0	Mean Temperature.	+62	+0	130
−10	* Temperature of Springs in England.	+52	−7	120
−30	Water Freezes.	+32	−20	100
−64	* Greatest Cold in England.	−2	−43	66
−101	Quicksilver Freezes.	−39	−67	29
−112	Greatest Natural Cold observed.	−50	−75	18
−130	Greatest Cold Natural.	−68	−86	0
−153	Greatest Cold Artificial.	−91	−102	−23
A		B	C	D

LXIII. *On*

LXIII. *On the Properties of Furze or Whins. By Major* Spencer Cochrane, *of Muirfield-House, near Haddington* *.

Sir, The Society having honoured me by publishing in their 25th volume my communication, stating the advantages arising from the culture of poppies, and that seven ounces of fine salad oil were furnished by expression from two pounds of the seed; I now beg leave to add, that I am informed, considerable quantities of poppy seeds have been lately bought up, in different parts of the country, and the expressed oil from them sold at the price of Florence oil; and that emulsions made from poppy seeds answer in every respect the purposes of those made from almonds.

The following communication may perhaps be deemed worthy the notice of the society; it relates to the use of Whins or Furze. Its utility as food for cattle has been long known, though probably not sufficiently appreciated; but as a medicine, I never till within a few years heard of it. My information was first received from a gentleman who has been an officer in the army, a friend and relation of mine; he is seventy-five years of age, and in good health, and what he says may be depended upon. In October 1806, he informed me that his sight had been much strengthened by drinking an infusion of whin or furze blossoms, dried in the sun in summer. The infusion is made from a tea-cup full of the blossoms, in a tea-pot in the manner of tea, and the dose half a tumbler at night; that he never had a cough since he first used it, which was fifty years ago; it acts as a diuretic, and by perspiration, and, when the dose is increased, promotes sleep. In October 1808, he informed me that he still continued the use of the whin-tea, that he had no cough, and that his skin was remarkably fine and soft, which he attributes to its use.

I have also used the whin blossoms with good effect myself, and can safely recommend them.

My friend supposes the young shoots of furze may answer if the blossoms cannot be got; he informs me, that when an epidemical cold came from Germany, and destroyed many horses in England, the east wind continued six weeks, and the infection came over to Ireland, where he had the care of a troop, in so poor a village that he could get

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1809.

neither

neither bran nor malt for mashes, which were ordered for the horses with sulphur, after bleeding: That he ordered the men to cut furze, and ordered them to give it to the horses, after they had beat it well on the pavement: that at first they had to mix it with oats, but that in two days the horses devoured it like clover. That by these means he recovered them all, though every other troop lost two or three; and that his was the only troop in good condition at the review. I remain with esteem, sir,

Your sincere and humble servant,

Muirfield, Jan. 22, 1809. SPENCER COCHRANE.

To C. TAYLOR, M.D. Sec.

LXIV. *Additional Observations for the Purpose of ascertaining the Value of growing Timber at different and distant Periods of Time. By Mr.* CHARLES WAISTELL, *of High Holborn, London* *.

SIR, IN the Society's last volume, under the head of Agriculture, are some tables and observations of mine, on the growth of timber †; and I have given one instance of six acres of very bad land, planted with Scotch firs under my directions, which at 29 years growth, and at the small price of 1*s.* a foot, had paid the owner 5*l.* per acre per annum compound interest. My motive in communicating these tables, observations and facts to the public, was to promote the planting of inferior and almost useless soils, in order to obtain from them timber of our own growth, sufficient for at least many of the purposes for which foreign timber is imported at an immense annual expense. For instance, much of such inferior soils will be found on Connock Heath in Staffordshire, on the moor lands in the north of that county, and on the moors in Derbyshire, Yorkshire, and northwards to Scotland, also on Bagshot Heath, Salisbury Plain, the heaths and wastes in Sussex, Hampshire, and Dorsetshire, and in many other counties in England, and also in Scotland and Wales. In that paper I suggested, that information of very great value on the subject of planting, might be obtained from noblemen and gentlemen to whom the Society had given medals and premiums for planting trees, if they would communicate to the Society their subsequent observations on such plan-

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1809.

† See Philosophical Magazine, vol. xxxiii.

tations;

lations; but it is to be regretted that the Society have not yet received any communications on the subject.

I have been solicited to republish my tables, &c. by different gentlemen, who I have reason to believe are very competent to judge of them, and I have in consequence several additional tables in forwardness with this view; but, previous to such publication, I wish to be furnished with a great variety of facts as to the growth of timber, and the management of plantations; and I shall feel much obliged to any persons who will have the goodness to send me their observations and statements of facts, respecting the growth and management of plantations; or, if they prefer it, they may send them to you at the Society of Arts, &c. The names of those who send such statements will be either given or suppressed as may be requested.

I will recapitulate nearly in the words of my former communication, several particulars on which information is wanted. It would, for instance, be desirable to have the nature of the soil and under strata described, on which plantations have been made; its value per acre; the mode in which it was prepared for planting; the sorts of trees planted thereon, and which of them were found best suited thereto; the distances at which the trees were first planted; at what periods they were thinned, and how many cut out at each thinning; and their measure and value; the present height, distance, measure and value of the trees now growing on an acre; what distances are found most advantageous; also to what proportion of their heights they should be pruned up, and the best and most expeditious mode of performing the operation.

Great loss is frequently sustained by omitting to thin plantations properly, and in due time, but I am not in possession of facts to calculate with accuracy what this loss may be; I will however venture to give a short statement of some calculations I have made, as to the loss that would now be sustained by letting trees grow to a great age.

In Miller's Gardener's Dictionary, it is stated that in a fall of oak timber in Lord Bagot's woods, Mr. Marshall counted the rings of one tree, which was sound at the butt, and found the number to be about 200. Its bole was 22 feet long, and 108 inches in circumference in the middle. Its contents 110 feet, which at 2*s.* amounts to 11*l.* I think it was last year, that a fine sound oak-tree was cut down, between Shrewsbury and Oswestry in Shropshire, of 300 years of age, and sold by auction for 52*l.* 5*s.*—And under my direction, many oak trees were cut down, some years

years ago, that could not be less than 300, and some of them probably 400 years of age, and even more. In Hunter's Evelyn's Silva is given the circumference of 10 trees, and not one of which was probably less than 500, and some of them probably 1000, years old.

Lord Bagot's tree of 200 years old, above mentioned, would, at the present price of 3*s.* a foot, be worth 16*l.* 10*s.* Supposing that 3*s.* a foot should continue to be the price of oak-timber for the next 200 years, we will inquire what sum might be raised by growing four oak-trees in succession, upon the same spot of ground, each tree to be cut down when 50 years of age, and that their boles should be of the same length as that of Lord Bagot's, viz. 22 feet.

I fix on fifty years of age, as being convenient for my calculation; and nearly the most profitable period at which to cut down trees of 22 feet bole, which have grown at the medium rate of one inch in circumference, and 12 inches in height annually.

After its 52d year, *such* a *bole* ceases increasing, after the rate of 5 per cent. per annum * : but the whole tree, including the top part above the bole, may continue increasing after that rate until its 61st year †.

I do not fix on 50 years of age as being the most profitable age at which to cut down trees; probably 60 or 70 years of age would in some instances be preferable. Supposing an oak-tree has increased as above mentioned, its bole of 22 feet would, at 50 years of age, measure 39 inches in circumference at the middle, and one-fourth of this, namely 9¾ inches, squared and multiplied into 22 feet, its length gives 14 feet 6 inches for its contents, which at 3*s.* a foot, its present value, amounts to 2*l.* 3*s.* 6*d.* Supposing 2*l.* 3s. 6*d.* to be the value of each of the four trees of 50 years of age, grown in succession upon the same spot of ground, in the period of 200 years, we will calculate to what the three first trees would amount, if their value was placed out at compound interest, for the respective terms of 150, of 100, and of 50 years.

£.	s.	d.		£.	s.	d.
2	3	6	Accumulating during 150 years, at 5 per cent. per annum compound interest, will amount to -	3,280	0	0
2	3	6	Accumulating as above for 100 years, would amount to -	286	0	0

* See Table 12 of a bole of 24 feet, in the 26th volume of the Society's Transactions, page 68.

† See my first Table in ditto, page 49.

Accu-

£. s. d.		£. s. d.
2 3 6	Accumulating as above for 50 years, will amount to -	24 0 0
	Add the value of the tree to be cut down at the end of 200 years	2 3 6
	Total amount in 200 years —	3,592 3 6
	And carrying forward this calculation, the total amount of the produce in 300 years would amount to —	£.472,408 0 0

In former times, when the value of oak-woods was estimated by the number of hogs their acorns would fatten, the great age of trees would be of small consideration; but in the present times, I am persuaded, that if gentlemen who have many trees standing of the age of 150 years and upwards, would give this subject its due consideration, they will be aware of the immense loss to which they are voluntarily subjecting themselves.—And this great loss is much to be regretted, in a political point of view, especially as the produce of this island is insufficient for its necessary consumption.

My motives for troubling you with this hasty production are, to promote the good of the public, by endeavouring to persuade gentlemen to bring forward well ascertained facts, respecting the most profitable management of growing timber trees, and to induce them to investigate, with accuracy, this very curious and important subject. If you think it is likely to have those effects, I shall thank you to lay this paper before the Society of Arts, &c. for their consideration.

I am, sir, your obedient servant,
CHARLES WAISTELL.

No. 99, High Holborn,
Oct. 1809.

LXIV. *A List of about Five Hundred Collieries in and near to Derbyshire. By Mr.* JOHN FAREY, *Mineralogical Surveyor.*

To Mr. Tilloch.

SIR, I INCLOSE a list, of such collieries as I have either visited or obtained information concerning, in the course of my recent examination of the county of Derby, and the borders of the seven adjacent counties. Should you deem the

the same worthy of a place in your Philosophical and Geological Magazine, it may be proper to observe, that the names of the places in the first column, are not always those of the parish in which the coal-works are situate, but of the nearest or most convenient place, shown in general maps, by which to refer to the precise site of the collieries. Also, that many of these works are now discontinued: yet as in almost every instance, more of this valuable mineral remains still ungotten, in or near to the same spot, the recording of all places where coals have at any time been worked, seems an object of some importance. In the report on the county of Derby, which I am at present employed in preparing, for the Board of Agriculture, I intend to give in one alphabetical list, the bearings and distances of each of these collieries, from the towns mentioned in the first column, with the particular place in the series of strata to which each colliery is to be referred, as far as I am able in the present instance: the collating of the great mass of information which I have been favoured with, from the several coal-masters, and their agents and workmen, being yet unfinished, as well as the reexamination of some parts of the interesting coal-fields, within the limits of my intended map, in districts where the alluvial coverings, the faults and stupendous dislocations of the strata, have presented great, but as I trust, not insuperable difficulties, to the complete elucidation of these highly important strata. In the report I shall distinguish such of the above collieries as now are, or recently have been, in work. Where no county is mentioned to the places in the first column, Derbyshire is to be understood; in which county I find the coal strata or measures, distributed over not less than 190,000 acres of its surface! I shall esteem it a great favour, if proprietors of estates, or others, who happen to be possessed of correct accounts of the sinkings or measures at any of the collieries below, in documents to which I may not have had access, or respecting any ancient collieries within these limits, which may have eluded my inquiries, that they will communicate copies of such particulars (addressed as below) as soon as convenient; carefully distingishing the bearing and distance from one at least of the above places; the time of ceasing to work, old collieries, if known, &c. The borings or sinkings, where trials to any depth have been made, whether successful or not, would be alike acceptable, such being the materials from which, principally, a correct account

count of the subterranean geography of the district, or the knowledge of its strata, is to be drawn.

I am, sir,
Your obedient servant,
JOHN FAREY.

12, Upper Crown Street, Westminster, June 5, 1810.

Places' Names.	Names of Collieries.
Alfreton	Alfreton, Four-lane-ends, Green-hill-lane, High-field-lane, Nether-Birchwood, Oakerthorpe, Riddins, Swanwick-green, Swanwick-delves, Somercotes, Somercotes-furnace.
Ashby-de la-Zouch, Leicestershire ...	Alton-grange, Southwood.
Ashover	Alton, Birkin-lane.
Ashton-under-line, Lancashire	Car-lane, Crickety, Fairbottom, Hays, Hurst-brook, Knoll.
Aston, Yorkshire ..	Bigging, Conduit-moor, Fox-Ears, Lawn (or Casteven or Kesteven), Pidgeon-bridge, Aston-common, Swallow-nest.
Attercliff, Yorks...	Attercliff-common, Bright-side, Darnall, High-hazels, Tinsley-park, Washford.
Awsworth, Notts.	Awsworth, Newthorpe-common.
Barlborough	Barlborough-common, Beighton-field, Hazlewell, Westfield, Knitaker, Pebley-lane, Spinkhill-common.
Barlow	Barlow-common, Cutthorpe, Far-lane, High-ash, Sudbrook, Wilders-green.
Barnsley, Yorks...	Gober-hall.
Baslow	Baslow, Chatsworth old Park.
Beaudesert - Park, Staffordshire ...	Egerton-wood.
Beeley	Beeley-moor, Harwood-Grange.
Beighton	Berley-moor, Nether-field.
Belper	Belper-gutter, Belper-lane-end, Belper Town, Bent, Chevin-side, Dally-gutter, Hopping-hill, Open-wood-gate, Swinney.
Biddulph, Staff...	Biddulph-hall, Crabtree, Childer-play, Falls, New-Pool.

Places' Names.	Names of Collieries.
Bilborough, Notts.	Bilborough, Holly-wood, Nuthall.
Blackfordby, Leices.	Blackfordby, Norris-hill.
Blackwell	Blackwell, Dimmings-dale, Newton.
Bolderstone-chapel, Yorkshire	Stock-bridge.
Bolsover	Palterton, Shuttlewood-common, Stanfrey.
Boothorpe, Leicestershire	Little-worth, Milk-hill, Sweet-hill-oak.
Bradfield-Chapel, Yorkshire	Hagen-field, Holes.
Brimington	Brimington, Brimington-moor, Hollingwood-common, Wildens-mill.
Buxton	Goyte-moss, Thatch-marsh.
Calke	Brians-coppy.
Chapel-en-le-Frith	Bugsworth, Cowpasture, Fernylee, Shallcross.
Chapel-Town, Yorkshire	Chapel-town, Heasley-Park, Parkins-wood, Smithy-wood-engine, Thorncliff.
Cheadale, Staff. ..	Delph-house, East-wall, Eaes, Movberley, Shaw (or Sham), Shaw, Woodhead.
Cheddleton, Staff. .	Crown-point, Cunsal-wood, New-stead, Shafferlong, Wetley-moor.
Chesterfield	Ash-gate, Boythorpe, Chesterfield-town's-end, Chesterfield-furnace, Calow, Dunston, Grass-hill, Hady, Hasland, Little-common, Lounsley-green, Moor-top, New-Brampton, Newbold-common, Newbold-field, Stone-gravel, Tapton, Walton, Wingerworth-park, Wingerworth-furnace.
Church-Gresley ...	Gresley, Gresley-hall, Round-hole, Woodwards.
Clown	Clown.
Codnor	Benty-field, Codnor nether-park, Codnor upper-park.
Cole-Orton, Leicest.	Cole-Orton, Lount new, Lount old.
Conisborough, Yorks.	Conisborough, Dennaby.
Cossall, Notts.	Cossall, Robinets.
Crich	Plaistow-Green.
Dale-Abbey	Dale-Abbey, Hag, Lower-Hag, Pingle.

Denby

Places' Names.	Names of Collieries.
Denby	Denby, Denby-hall, Roby east-field, Roby west-field, Smithy-houses.
Dilhorne, Staff. ..	Dilhorne, Parsons-field, Swetley (Lowes').
Disley, Cheshire ..	Allington, Bank-end, Diglee, Furnace-clough, Gee, Hag-bank, Hoo-lane, Lyme, Lyme-park, Norbury, Poynton, Red-acre, Worth.
Dore	Dore, Ringing-low-bar.
Dronfield	Apperknowl-common, Cole-Aston, Dronfield, Hill-top, Ounston, Stubley, Woodhouse.
Duckingfield, Ches.	Duckingfield, Dunkirk, Flowery-field, Hough-hill, Newton-moor, Rabbit-hole, Score-wood.
Duckmanton (long)	Adelphi-furnace, Duckmanton-common, Middle-Duckmanton.
Eastwood, Notts. .	Beggerlee, Brinsley new, Brinsley old, Eastwood.
Eccleshall Barlow, (in Sheffield,) Yorkshire	Grey-stones, Mill-house, Moss, Smelting, Trap-lane, Whitley-wood.
Eckington	Arbor-lands, Bole-hill, Bramley-moor, Coldwell, Eckington, High-lane, Mossborough-moor, Troway.
Flash (in Alston-field), Staff.	Birchen-booth, Black-clough (or Beat), Blue-hills, Chest, Dane-head, Dane-thurn, Diamond-hill, Gold-sitch, Hazle-barrow, Notbury, Penny-hole, Whiteshaw.
Foxton, Staffordsh.	Clough-head, Foxton-wood, Stile-shutt.
Fullwood-Chapel, (in Sheffield) Yorks.	Stanage.
Glossop	Combs, Simondley.
Greasborough, Yorks.	Cinder-hill (or Middle-field), Haw-wood.
Greasley, Notts. ..	Beauvale-abby, Greasley, Limes.
Hansworth, Yorks.	Ballyfield, Gleedless-common, Hansworth, Hansworth-woodhouse, Intake, Orgrave, Woodthorpe.
Harthill, Yorks. ..	Woodhall-moor.
Hartshorn	Gosley-waste, Hartshorn.
Hathersage	Stanage-pole.

Hay-

Places' Names.	Names of Collieries.
Hayfield (in Glossop)	Aspinshaw, Burn'd-edge, Moor-top.
Heage	Heage, Heage-bent, Morley-park, Town-field.
Heanor	Aldercar, Heanor, Langley, Milnhay, Shipley.
Heath	Heath, High-house.
Heather, Leicester.	Heather.
Higham	Higham.
Holbrook.	Holbrook.
Holmesfield	Bank, Salters-sitch, Thickwood.
Horsley	Horsley, Horsley-woodhouse, Slack-fields.
Hyde-Chapel, Cheshire	Broomsteer, Denton, Hyde-lane, Werneth-low, Woodley.
Ilkeston	Cotmanhay-wood, Ilkeston, Ilkeston-common, Little-Hallam.
Ipstone, Staff.	Crow-gutter, Hay-house, Ipstone, Knipe, Nether-field.
Killamarsh	Gander-lane, Killamarsh, Nether-moor, Killamarsh old-delph, Over-thorpe.
Kimberworth, Yorks.	Blackburn-bank, Bradgate, Kimber-worth, Meadow-hall.
Kingsley, Staff.	Froghall, Garstone, Hazles-cross, Hodge-hay, Jack-elm, Kingsley-bank, Lees, Rake-edge, Ross-bank.
Macclesfield, Ches.	Blake-low, Bollington, Cliff-bank, Eastborough-lane, Hurdsfield, Macclesfield-common, Riley-clough, Shrigley-fold, Swanco, Throtles-nest.
Marple-Chapel, Ches.	Brabins, Chapel-house.
Matlock	Lea, Lumsdale, Tansley-green.
Measham	Donisthorpe, Measham, Measham-fields, Oakthorpe.
Mellor (in Glossop)	Bore-lane, Broadhurst-edge, Compstal-bridge, Ludworth, Old-hall-wood, Shaw-hay.
Mexborough, Yorks.	Mexborough.
Morley	Morley.
Mossley, Lancash.	King-bank, Park, Rotches, Scout-mill, Windy-bank.
Mottram, Cheshire.	Hague-bank, Hill-end, Hodge-hall.

Newall

Places' Names.	Names of Collieries.
Newall (in Stapenhill)	Bretby, Hall-fields, Newall, Newall-park, Perkins, Stanton, Swadlincote, Water-field, Wooden-box, Wood-field.
New-Mills (in Glossop)	Eaves-knowl, Lower-house, Tormine, Warps-moor.
Norton, Staff.	Bow-green, Whitfield.
Norton	Lees-hall.
North-Winfield ...	Ankerbold, Berrisford-moor, Clay-cross, Henmore, Loco-lane, Pilsley-lane, Tupton-green, Woodthorpe.
Oakmoor-Mills (near Alveton) Staff.	Beelow, Car-wood.
Over-Seal, Leicest.	Warren-hill-furnace.
Packington, Leicest.	Packington.
Pelsall (near Bloxwick) Staff.	Brown-hills, Essington new Colliery, Essington-wood, Goscot, Lords-hay, Pelsall, Wyrley-bank.
Penistone, Yorks. ..	Bull-house, Flash-house, Fullshaw, Law, Midhope-stones, Paw-hill.
Pentrich	Castle-hill, Harts-hay, Pentrich.
Pinxton	Carter-lane, Pinxton.
Pott-Shrigley, Chesh.	Bakestone-dale, Berristow, Harrop, Pott-hall, Spons-moor, Spons, Styperson.
Rainow-Chapel, Chesh.	Kerridge-east-side, Kerridge-north-end, New-post, Rainow-low.
Rawmarsh, Yorks.	Nether-Hough, Over-Hough, Rawmarsh, Stubbing-lane.
Ripley (in Pentrich)	Butterly-park, Butterly-car, Greenwich, Ripley.
Rotherham, Yorks.	Clough, Herringthorpe, Hill-top, Kimberworth-park, Mossborough-common.
Rudgley, Staff. ..	Bruerton.
Sheffield, Yorks. ..	Crooks-moor, Deep-pits, Harbour-thorn, Manour, Park-furnace, Ponds, Sandy-gate.
Shirland	Shirland, Smithy-moor, Stretton.
Silkstone, Yorks. ..	Silkstone.
Skegby, Notts.	Shilo, Dirty-Hucknale, Skegby.
Smalley	Smalley, Smalley-common, Simon-field, Woodhouse-lane.

Places' Names.	Names of Collieries.
Smithsby	Pistern.
South-Normanton .	Berristow, South-Normanton.
Stannington, Yorks.	Armitage, Dungworth, Low-ash, Storrs, Wadsley.
Stanton (by Dale) .	Hallam-bridge (or Nutbrook).
Stanton-Harold, Leicester.......	Heath-End, Stanton-Harold.
Stapleford, Notts..	Bramcote.
Staveley	Norbrigs, Staveley, Woodthorpe.
Sutton (in Scarsdale)	Sutton, Sutton-common, Wood-nook.
Swepston, Leicester.	Swepston.
Tamworth, Warwickshire	Polesworth, Waverton, Wilnecote.
Tankersley, Yorks.	Tankersley-park.
Taxhall, Cheshire .	Castedge, Gap-sitch.
Temple-Normanton	Grass-moor, Grass-moor (Platt's), Lings.
Teversall, Notts...	Dunshill.
Thursfield, Staff. .	Bernersley-green.
Ticknall	Ticknall, White-holly-coppy.
Todwick, Yorks. ..	Todwick-moor.
Tibshelf.........	Biggin, Harstoft, Tibshelf.
Treton, Yorks.....	Cat-cliff.
Wales, Yorks.....	Wales.
Wath, Yorks......	Abdy, Wath-wood.
Wentworth-Chapel, Yorks.........	Cortworth, Elsicar, Hoober, Low-wood, Park-gate, Swallow-wood-nook, Wentworth-park.
West-Hallam.	Stanley-common, West-Hallam, West-Hallam windmill-hill.
Whiston, Yorks. ..	Royds-moor.
Whittington	Glass-house-common, Whittington-moor.
Wickersley, Yorks..	Brecks, Hollings-moor.
Wincle-Chapel, Cheshire	Green-hill, Hay, Latche, Mouse-trap, Robins-clough, Quarnford.
Wirksworth........	Alderwasley, Wigwell.
Wollaton, Notts. ..	Aspley, Trowel-moor, Wollaton.
Woodhead, Cheshire	Crowden-clough.
Worsborough, Yorks.	Stainborough-park, Worsborough.
Wortley-Chapel, Yorks..........	Deep-car, Hunshelf, Westwood.

LXVI. *The*

LXVI. *The Croonian Lecture.* By WILLIAM HYDE WOLLASTON, *M.D. Sec. R.S.**

I AM aware that the remarks, which I have to offer on the present occasion, may be thought to bear too little direct relation to each other for insertion in the same lecture; yet any observation respecting the mode of action of voluntary muscles, and every inquiry into the causes which derange, and into the means of assisting the action of the heart and blood-vessels, must be allowed to promote the design of Dr. Croone, who instituted these annual disquisitions. And it has always appeared to be one great advantage attending the labours of this society, that it favours the production of any original knowledge, however small, in a detached form; and enables a writer to say all that he knows upon a particular subject, without inducing him to aim at the importance of a long dissertation.

I shall therefore make no apology for dividing the following lecture into three distinct parts.

In the first of which I shall treat of the duration of voluntary action.

In the second, I shall attempt to investigate the origin of sea-sickness, as arising from a simple mechanical cause deranging the circulation of the blood.

In the third, I shall endeavour to explain the advantage derived from riding, and other modes of gestation, in assisting the health under various circumstances, in preference to every species of actual exertion.

Part I. *On the Duration of Muscular Action.*

The necessity of occasional intermissions from a series of laborious exertions, is within the experience of every one; the fatigue of continuing the effort of any one voluntary muscle without intermission even for a few minutes is also sufficiently known; but there is a third view of the duration of muscular action which appears to have escaped the notice of physiologists; for I believe it has not hitherto been observed that each effort, apparently single, consists in reality of a great number of contractions repeated at extremely short intervals: so short indeed that the intermediate relaxation cannot be visible, unless prolonged beyond the usual limits by a state of partial or general debility.

* From Philosophical Transactions for 1810, Part I.

I have been led to infer the existence of these alternate motions from a sensation perceptible upon inserting the extremity of the finger into the ear. A sound is then perceived which resembles most nearly that of carriages at a great distance passing rapidly over a pavement.

The rapidity of the motion varies according to the degree of force with which the finger is retained in its place. The sound thus perceived is not at all dependent on the degree of pressure upon the tympanum; for, on the contrary, the vibratory sound is most distinct when that pressure is slight, if the finger be at the same time rendered rigid by the forcible action of antagonist muscles; and when the ear is stopped with great force without the presence of muscular action, no such sound is produced. For instance, if the head be rested upon the hand in such a position, as to press with its whole weight upon the ball of the thumb applied to the ear, no noise is perceived, unless the extremity of the thumb be at the same time pressed against the head, or unless the action of some other muscles be communicated to the ear, by any inadvertence in the method of conducting the experiment.

When I endeavoured to estimate the frequency of these vibratory alternations, they appeared to be in general between 20 and 30 in a second; but it is possible that the method I employed may be found defective, and it is to be hoped that my estimate may be corrected, by some means better adapted to the determination of intervals that cannot actually be measured.

It was by imitation alone that I was enabled to judge of their frequency. For this purpose I contrived to render the vibration itself, and the imitative sound, both audible by the same ear.

While my ear rested on the ball of my thumb, my elbow was supported by a board lying horizontally, in which were cut a number of notches of equal size, and about one-eighth of an inch asunder. Then, by rubbing a pencil or other round piece of wood with a regular motion along the notches, I could imitate pretty correctly the tremor produced by the pressure of my thumb against my head; and by marks to indicate the number of notches passed over in five or ten seconds, observed by my watch, I found repeated observations agree with each other as nearly as could be expected; for I could not depend upon exerting the same degree of force in different trials.

That I might not be deceived by the resemblance of tremors, which coincided only at alternate beats, and therefore

fore might be considered as octaves in music to each other, I sometimes employed notches at greater and sometimes at less distances from each other, but the result was nevertheless the same; and in order to avoid any error that might be caused by some accidental quality of the sound arising from the length of the muscle employed, or length of the bones concerned in conveying the imitative sound to my ear, I made the following variation of the experiment. My ear was stopped by a cushion pressed upon by the end of a notched stick that rested on my foot, and thus conveyed the vibration from the muscles of my leg to the ear, along with the tremor produced by friction upon the notches; and still the results were nearly the same; varying in frequency between 20 and 30 in a second, according to the degree of force exerted in the experiment *.

As a further proof that I was not much deceived in my judgement of the frequency of these vibrations, I requested two or three of my friends to repeat the same experiment for me, and our agreement was such as to confirm me in opinion, that there could be no very considerable error in the estimate.

The greatest frequency that I think I have observed, was about 35 or 36 in a second, and the least was as low as 14 or 15; but in attempting to lessen the number of vibrations, there appears to be a degree of unsteadiness which prevents any accurate measurement of the real number.

It is very probable, that in cases of great debility the number may be even considerably less, and may be the reason of that visible unsteadiness, which is known to occur in persons enfeebled by age, or much reduced by disease.

Possibly the foregoing observation may not be altogether new to some members of this society, as it is now about 17 or 18 years since it first occurred to me, and I was then accustomed occasionally to mention it in conversation with my friends; but I am not aware that any other person has made the same remark respecting the vibratory nature of muscular action, although I find that Grimaldi had ob-

* The resemblance of the muscular vibrations to the sound of carriages at a distance, I apprehend to arise not so much from the quality of the sound, as from an agreement in frequency with an average of the tremors usually produced by the number of stones in the regular pavement of London, passed over by carriages moving quickly.

If the number of vibrations be supposed 24 in a second, and the breadth of each stone be about six inches, the rate of a carriage thus estimated would be about eight miles an hour; which agrees with the truth as nearly as the assumptions on which the estimate is founded.

served

served the sound that occurs upon stopping the ears, but ascribed it, according to the notions that prevailed in his time, to the hurried motions of the animal spirits*.

Part II. *On Sea-Sickness.*

The second remark which I have to offer to the society relates to sea-sickness, the cause of which has not hitherto been fully explained; and although the explanation which I am about to propose, may not appear altogether satisfactory to persons who, when at sea, are also rendered giddy by the incessant motion of the waves, and are consequently liable to consider as cause and effect phænomena which in their minds are constantly associated; yet the observation on which it is founded may deserve to be recorded, on account of the degree of relief that may be obtained in that most distressing affection.

After I had been harassed by sea-sickness during a short voyage for some days, and had in vain attempted to account for the difference between the inexperienced passenger, and those around him more accustomed to the motion of the sea, I imperceptibly acquired some power of resisting its effects, and had the good fortune to observe a peculiarity in my mode of respiration, evidently connected with the motion of the vessel, but of which, in my then enfeebled state, I was unable to investigate either the cause or consequence. In waking from a state of very disturbed sleep, I found that my respirations were not taken with the accustomed uniformity, but were interrupted by irregular pauses, with an appearance of watching for some favourable opportunity for making the succeeding effort; and it seemed as if the act of inspiration were in some manner to be guided by the tendency of the vessel to pitch with an uneasy motion.

The mode by which I afterwards conceived that this action could primarily affect the system, was by its influence on the motion of the blood; for, at the same instant that the chest is dilated for reception of air, its vessels become also more open to the reception of the blood, so that the return of blood from the head is more free than at any other period of a complete respiration. On the contrary, by the act of expelling air from the lungs, the ingress of blood is so far obstructed, that, when the surface of the brain

* Vera itaque ratio experimenti prædicti est, quia in digito et brachio totoque corpore continuato fiunt multi motus ac tremores, ob spirituum agitationem huc illuc perpetuo accurrentium. *Grimaldi*, Physicomathesis de Lumine, p. 383.

brain is exposed by the trepan, a successive turgescence and subsidence of the brain is seen, in alternate motion with the different states of the chest. It is probably from this cause that, in severe head-aches, a degree of temporary relief is obtained by occasional complete inspirations.

In sea-sickness also the act of inspiration will have some tendency to relieve, if regulated so as to counteract any temporary pressure of blood upon the brain; but the cause of such pressure requires first to be investigated.

All those who have ever suffered from sea-sickness (without being giddy) will agree that the principal uneasiness is felt during the subsidence of the vessel by the sinking of the wave on which it rests. It is during this subsidence that the blood has a tendency to press with unusual force upon the brain.

If a person be supposed standing erect upon deck, it is evident that the brain, which is uppermost, then sustains no pressure from the mere weight of the blood, and that the vessels of the feet and lower parts of the body must contract, with a force sufficient to resist the pressure of a column of blood, of between five and six feet from the head downwards.

If the deck were by any means suddenly and entirely removed, the blood would be no longer supported by its vessels; but both would fall together with the same velocity by the free action of gravity; and the same contraction of the vessels which before supported the weight of the blood would now occasion it to press upon the brain, with a force proportional to its former altitude.

In the same manner, and for the same reason, during a more gradual subsidence of the deck, and partial removal of support, there must be a partial diminution of the pressure of the blood upon its vessels, and consequently a partial reaction upon the brain, which would be directly counteracted by a full inspiration.

The consequence of external motion upon the blood will be best elucidated by what may be seen to occur in a column of mercury similarly circumstanced.

A barometer, when carried out to sea in a calm, rests at the same height at which it would stand on shore; but, when the ship falls by subsidence of the wave, the mercury is seen apparently to rise in the tube that contains it, because a portion of its gravity is then employed in occasioning its descent along with the vessel; and accordingly, if it were confined in a tube closed at bottom, it would no longer press with its whole weight upon the lower end. In

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the same manner, and for the same reason, the blood no longer presses downwards with its whole weight, and will be driven upwards, by the elasticity which before was merely sufficient to support it.

The sickness occasioned by swinging is evidently from the same causes as sea-sickness, and that direction of the motion which occasions the most piercing sensation of uneasiness, is conformable to the explanation above given.

It is in descending forwards that this sensation is perceived; for, then the blood has the greatest tendency to move from the feet towards the head, since the line joining them is in the direction of the motion. But when, in the descent backwards, the motion is transverse to the line of the body, it occasions little comparative inconvenience, because the tendency to propel the blood towards the head is then inconsiderable.

The regularity of the motion in swinging, afforded me an apparently favourable opportunity for trying the effect of inspiration; but although the advantage was manifest, I must confess, it did not fully equal the expectations I had formed from my experience at sea. It is possible that the suddenness of the descent may in this case be too great to be fully counteracted by such means; but I am inclined to think that the contents of the intestines are also affected by the same cause as the blood; and if these have any direct disposition to regurgitate, this consequence will be in no degree counteracted by the process of respiration.

A friend of mine informed me that he had endeavoured to counteract this mechanical effect upon the stomach, and had experienced immediate relief from a slight degree of sea-sickness, by lying down upon the deck with his head towards the stem of the vessel; by means of which, upon pitching, he was in the attitude of a person descending backwards in a swing.

Whether the stomach be or be not thus primarily affected, or only by sympathy with the brain, the sensation of sinking is in all cases referred directly to the stomach, which is seized with such instantaneous retching, that no person who has not been so situated can form a just conception of it*.

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* There is one occasion upon which a slighter sensation of this kind is perceived, and it appears to indicate the direction of the motion from which it arises, to be downwards. "In a country subject to frequent returns of earthquakes," it is said† that "a few minutes before any shock came, many

† Phil. Trans. vol. xlii. p. 41.

In thus referring the sensations of sea-sickness in so great a degree to the agency of mere mechanical pressure, I feel confirmed by considering the consequence of an opposite motion, which, by too quickly withdrawing blood from the head, occasions a tendency to faint, or that approach to fainting, which amounts to a momentary giddiness with diminution of muscular power. At a time when I was much fatigued by exercise, I had occasion to run to some distance, and seat myself under a low wall for shelter from a very heavy shower. In rising suddenly from this position I was attacked with such a degree of giddiness, that I involuntarily dropped into my former posture, and was instantaneously relieved, by return of blood to the head, from every sensation of uneasiness.

Since that time, the same affection has frequently occurred to me in slighter degrees, and I have observed, that it has always been under similar circumstances of rising suddenly from an inclined position, after some degree of previous fatigue. Sinking down again immediately removes the giddiness; and then, by rising a second time more gradually, the same sensation is avoided.

Part III. *On the salutary Effects of Riding, and other Modes of Gestation.*

In the preceding instances of disturbing the circulation of the blood, by external motion, the effect is disagreeable, and proportionally prejudicial. There may indeed be cases of disorder, in which it will be salutary, but these are probably less frequent than is generally supposed.

In the observations which follow, general opinion will concur with me, on the benefit derived from external or passive motion, and I hope that, in ascribing its good effects to their true cause, I shall enable others to make a valuable distinction, which has not yet been preserved with due care, between one motion which is salutary, and another which is very frequently pernicious. For, although the

many people could foretel it by an alteration in their stomachs; an effect which (it is added) always accompanies the wave-like motion of earthquakes, when it is so weak as to be uncertainly distinguishable." (Michell, Phil. Trans. vol. li. 610.)

It seems that the vapours to which these tremendous concussions are owing, immense in quantity, and of prodigious force, being for a time confined on all sides, elevate the surface of a country to a vast extent until they either find vent, or meet with some partial cause of condensation; and hence the alternate heaving and subsidence of the ground will produce much the same effects as the rising and falling of the swell at sea.

the term *gestation* is employed by medical writers, as a general term comprehending riding on horseback, or in a carriage, and although the merits of such motions, especially the former, were clearly noticed, and perhaps even over-rated, by the discernment of Sydenham, I believe that no explanation has yet been given, of the peculiar advantages of external motion, and am persuaded, that the benefits to be derived from carriage exercise are by no means in so high estimation as they ought to be.

Under the common term *exercise*, active exertion has too frequently been confounded with passive gestation, and fatiguing efforts have consequently been substituted for motions that are agreeable, and even directly invigorating, when duly adapted to the strength of the invalid, and the peculiar nature of his indisposition.

The explanation which I am about to offer of the effects of external motion upon the circulation of the blood, is founded upon a part of the structure observable in the venous system, the mechanical tendency of which cannot be doubted. The valves which are every where dispersed through those vessels, allow free passage to the blood, when propelled forward by any motion that assists its progress; but they oppose an immediate obstacle to such as have a contrary tendency. The circulation is consequently helped forward by every degree of gentle agitation. The heart is supported, in any laborious effort that may have become necessary, by some obstacle to its exertions; it is assisted in the great work of restoring a system, which has recently struggled with some violent attack: or it is allowed, as it were, to rest from a labour, to which it is unequal, when the powers of life are nearly exhausted by any lingering disorder.

In the relief thus afforded to an organ so essential to life, all other vital functions must necessarily participate; and the various offices of secretion, and assimilation, by whatever means they are performed, will not fail to be promoted during such comparative repose from laborious exertion.

Even the powers of the mind itself, though apparently least likely to be influenced by mere mechanical means, are manifestly, and in many persons most immediately, affected by these kinds of motion.

It is not only in cases of absolute deficiency of power to carry on the customary circulation, that the beneficial effects of gestation are felt, but equally so when comparative inability arises from redundancy of matter to be propelled.

propelled. When from fulness of blood the circulation is obstructed, the whole system labours under a feeling of hurry and agitation, with that sensibility to sudden impressions which is usually termed nervousness. The mind becomes incapable of any deliberate consideration, and is impressed with horrors that have no foundation but in a distempered imagination.

It is in moderate degrees of this species of affection that the advantages of carriage exercise are most sensibly felt. The composed serenity of mind that succeeds to the previous alarm, is described by some persons with a degree of satisfaction that evinces the decided influence of the remedy. With this steadier tone of mind, returns its full power of cool reflection; and if the imagination becomes more alive than usual, its activity is now employed in conceiving scenes that are amusing and agreeable.

As an instance of direct relief to a circulation labouring from mere fulness of blood, I may adduce that of a person, whose friends, as well as himself, were apprehensive, from the violent and visible throbbing of his heart, of the existence of some organic mischief, and were in some measure alarmed for the consequences.

He was persuaded, and not reluctantly, to go without delay for medical advice, and was accordingly conveyed in a carriage to the house of some physician of eminence, but did not succeed in finding him at home. As the symptoms did not appear to admit of delay, and were at least not aggravated by the motion, it was hoped that the wished-for advice might be obtained at a part of the town which happened to be at some distance. But the second attempt proved as fruitless as the former, and a third was made with the same event. Since the throbbing had by that time considerably abated, he was contented to postpone any further efforts to the following day, and directed the carriage homewards. By the time that he returned to his friends, he found that the motion of travelling over several miles of pavement had apparently removed the complaint. The pulsation of the heart and arteries had subsided to their natural standard, and he congratulated himself, that his search of a remedy had not been ineffectual, although he had been disappointed as to the source from which he thought he had most reason to expect relief.

If vigour can in any instance be directly given, a man may certainly be said to receive it in the most direct mode, when the important service of impelling forward the circulation of his blood is performed for him by external means.

means. The main spring, or first mover of the system, is thereby, as it were, wound up; and although the several subordinate operations of so complicated a machine cannot be regulated in detail by mere external agency, they must each be performed with greater freedom, in consequence of this general supply of power.

In almost every treatise on the subject of chronical diseases, are to be found numerous instances of the benefit produced by the several modes of gestation which have been most generally adopted; as riding on horseback, in carriages, sea-voyages, and swinging. And in many cases which might be adduced, it has appeared too clear to admit of a doubt, that the cure of the patient has been owing *solely* to the external agitation of his body, which must be allowed, at least, to have had the effect above explained; that of relieving the heart and arteries from a great part of their exertion in propelling the blood, and *may* therefore have contributed to the cure, by that means only.

The different modes above mentioned are adapted from their nature to different degrees of bodily strength; and if there are cases in which that which appears most eligible may not suit the situation or circumstances of the patient, it cannot be difficult to contrive other means of giving motion, so as least to incommode, and yet to give the greatest relief. A very gentle and long continued, or even incessant motion, may suit some cases better than any more violent and occasional agitation; and in this way, probably, it is, that sea-voyages have sometimes been attended with remarkable advantage.

LXVII. *Comparative Tables of the Beats of the Tempered Consonances in M. Kirnberger's and the Isotonic or Equal Temperament Systems of Tuning; with Remarks on the common System used by Organ Tuners, compared with that of M. Kirnberger.* *By the Rev.* C. J. Smyth, *Minor Canon of the Cathedral, Norwich.*

The opinions of profound theorists are ever entitled to attention; but should not be received with implicit faith. If such a man as Sir Isaac Newton was capable of a mistake, so is an Emanuel Bach, or a Kollmann. To the latter gentleman the musical world is under the greatest obligations for reducing the theory of Composition to a degree of

of simplicity, before his writings unknown. But the opinions he has advanced, with respect to the temperament of the musical scale, require that minute investigation and submission to calculation, which, if they had been advanced by a person of less celebrity, might have been passed over in silence.

One of the grand objects at which he appears to aim, is to establish an *equal temperament* on the piano-forte (that is, that all chords of the same kind shall be alike, as to their degree of imperfection): to this I raise no objection; the rage for modulation at present exerting its energies, to their utmost possible extent; a rage very favourable to the talents of those, who have not invention sufficient to produce novel and beautiful melodies, and yet aspire to the character of interesting composers.

I shall first presume to offer a few observations on the *unequal temperament* of Kirnberger, which Mr. Kollmann *supposes* "one of the best hitherto known." Not having the happiness to be able to read German, I know not what M. Kirnberger has offered in its behalf; or those important observations which major Templehoff (in an Essay in that language published in 1775, Berlin) is said by Dr. Robison in the Encyc. Brit., art. *Temperament*, to have made, on Kirnberger's system.

Preferring at all times experiment to theory, I tuned my piano-forte according to Mr. Kollmann's printed directions, with a view to hear the effect of one of "the best unequal temperaments." I cannot speak favourably of the result. I will below subjoin a table of *the beatings* of the tempered consonances, in order to give those professors who may feel no inclination to submit to the drudgery of calculation, some idea of what the effect of this temperament would be on an organ, where those *beatings* are most distinctly heard: a formidable host of foes, inimical to correct, and even tolerable tune. It appears to me, in the outset, doubtful whether a tempered system should have any *perfect* chords (but the octaves); as those chords, whenever they are heard, will render the ear less disposed to be pleased with the imperfect harmony which follows.

We will suppose the performer on the organ, to begin with Kirnberger's system in the key of C, answering to the tenor clif; here is a chord absolutely *perfect*; so also is the chord of G, the fifth of the key; but the chord of the fourth of the key ranks, in point of importance, next to the key-note and its fifth; and here unfortunately is a chord of which A, the major third to F, beats 149 times in

in 15″. From the key of C a modulation will naturally be expected into the key of G; which modulation will require the dominant of G, viz. the chord of D with a major third, fifth, and seventh. Now the A is half a comma too flat, as a fifth above D, and will beat 75 times in 15″. And this we may take, as the first instance of "a very fine variety of perfection." Surely Mr. Kollmann would have been more consistent in the use of terms, if he had said "varieties of imperfection." The next chord which occurs is that of E, the fifth of which is perfect, but the major third beats 255 in 15″. The chord of F has already been spoken of; proceed we then to examine the chord of B♭; the fifth is perfect, the major third beats 399. This chord on E♭ beats 266.

Now let us compare this temperament with that upon organs tuned in the usual manner. I do not give the beats in extreme keys, because they are too rapid to be heard as *beatings*, except in the lowest part of the scale, and have a *rough* effect, which is more tolerable than beatings not too rapid to be perceived as beatings. One chord, viz. that of A♭ or G♯ has a peculiar character, (it is called the *wolf*) the fifth being almost the fifth part of a minor tone *too sharp*, and the beatings are distinctly heard in the middle and lower part of the scale.

Chords on the Organ, as tuned in the usual Manner.

C, G, D, A, and E, good.
B, F♯, C♯ and G♯. The major thirds almost one fourth of a minor tone too sharp, and A♭ (or G♯) has also a fifth almost
F, B♭ and E♭, good. one-fifth of a minor tone too sharp, as before observed.

When we hear an organ tuned in this manner, we may consider ourselves at a feast, in which there are dishes of various qualities; while in M. Kirnberger's feast of *exquisite viands*, but *eight dishes* are very palatable, and those who are fond of sour crout and olives, and, like many of our best composers, have no objection to a slice of wolf, though they would not choose to dine entirely upon that outlandish animal, have an opportunity of gratifying their peculiar palates. Until, therefore, some irrefragable arguments are produced, to prove the superiority of M. Kirnberger's temperament to that in common use, I presume our organ-builders and organ-tuners will, in spite of any charges of obstinacy, ignorance, or policity, continue to tune as their ancestors did before them: as I cannot flatter myself the

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public will ever go to an enormous expense, for many additional pipes, in order that our old and young organists may perform their wonderful feats of modulation (which require as little genius as application) without torturing those who prefer tolerable tune, to the parade of science. Further observations, on mean-tone temperaments, may be offered hereafter.

C. J. SMYTH.

M. KIRNBERGER'S TEMPERAMENT, Beats in 15″.

(The Vibrations communicated by Mr. FAREY.)

Keys.	Vibrations in 1″.	3ds.	IIIds.	4ths.	Vths.	6ths.	VIths.
c	480						
B	450	0	383	0	0	0	383
Bb	426·6667	474	399	0	0	574	399
A	402·4922	224	153	150	112	299	188
Ab	379·2593	383	355	0	0	510	355
G	360	399	0	0	0	533	0
Gb	337·5	188	287	0	17	0	287
F	320	355	149	0	0	474	300
E	300	0	255	112	0	0	255
Eb	284·4444	287	266	0	0	383	266
D	270	300	0	0	75	399	0
Db	252·8395	255	237	17	0	153	237
C	240	266	0	0	0	355	112
		3131	2484	279	204	3680	2782
		2484					
		279					
		204					
		3680					
		2782					
		12560	Sum total.				

The Isotonic Scale*, Beats in 15″.

(The Vibrations communicated by Mr. Farey.)

Keys.	Vibrations in 1″.	3ds.	IIIds.	4ths.	Vths.	6ths.	VIths.
c	480						
B	453·0613	366	269	30	22	428	308
*	427·6307	345	254	28	21	403	291
A	403·6312	326	240	27	20	381	274
*	380·9784	308	226	25	19	360	259
G	359·5939	291	214	24	18	339	244
*	339·4127	274	201	22	17	320	230
F	320·3612	259	190	21	16	302	218
E	302·3819	244	180	20	15	285	205
*	285·4090	230	169	19	14	269	194
D	269·3913	218	160	18	13	254	133
*	254·2725	205	151	17	12	240	173
C	240	194	142	16	12	226	163
		3260	2396	267	199	3807	2692
		2396					
		267					
		199					
		3807					
		2692					
		12621	Sum total.				

LXVIII. *On Crystallography. By M.* Haüy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 363.]

GEOMETRICAL CHARACTERS OF CRYSTALS.

16. *Forms. Nucleus or primitive form.*

It is very rare to find a mineral under its primitive form given immediately by nature, and there is a certain number of species in which this form is known only from the results of mechanical division and by theory. The just measurement of actions susceptible of producing it is only as

* See our xxviiith volume, p. 65, and our xxixth volume, p. 347.—Edit.

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it were a point which frequently escapes in the process of crystallization, amid that multitude of circumstances which influence in so many ways the progress of this operation.

The diversity of the primitive forms ought to be regarded as a certain indication of a difference in nature between two substances, and the identity of primitive form indicates that of nature, at all times when this form is not one of those which have a marked character of regularity, such as the cube, the regular octahedron, &c.

Secondary forms. In order to describe more easily the secondary forms, we shall suppose them always situated in such a manner that the line which may be considered as their axis has a vertical position, and then the faces parallel to this axis will themselves bear the name of *vertical faces*; we shall call *horizontal faces* those which will be perpendicular, and *oblique faces* those which will be inclined towards it.

We are sometimes in the situation of indicating the incidence of a face which is presented in front in the projection of a crystal, on that which is adjacent to it behind the same crystal. We shall then give to the latter the name of *returned facet*. Suppose, for example, that in the distich topaz represented (fig. 61, Pl. VII) it is requisite to indicate the angle formed by one or other of the panes *o, o*, with that which is contiguous to it in the posterior part, we shall say that the incidence of *o* on the returned pane is 93° 6′.

The forms of crystals are subject to various kinds of alterations purely accidental. One consists in certain faces being nearer to, or more distant from, the centre in one crystal than in another which belongs to the same variety, in such a way, however, as constantly to preserve a certain character of symmetry. In several cases these variations only fall on the dimensions of the faces, and not on the number of their sides. This happens with certain dodecahedral garnets, which in the case of perfect symmetry would have their surface composed of twelve equal and similar rhombs, and which are lengthened in the direction of an axis which would pass by two of their opposite solid angles taken among those which are formed of three plane angles. The dodecahedron is then presented under the appearance of a solid with six panes which are elongated rhombs, with summits of three faces each which are true rhombs. In other cases, the faces themselves, or some of them at least, change their figure, by the increase or diminution of the number of

their sides. Thus, upon the hypothesis that the cube performing the function of the primitive form undergoes a decrement by a simple range around its eight solid angles, it may happen that the effect of the decrement remains interrupted, at the term at which all the faces which it produces are equilateral triangles far enough removed from the centre to avoid meeting, and then the faces parallel to those of the primitive cube will be octagons. If, on the contrary, the same faces come in contact, the primitive planes will preserve the form of the square: finally, if they intersect each other, they will be changed into hexagons, without the primitive faces ceasing to be squares, and these variations might pass through an infinity of degrees which will be as many approximations, with respect to the form of the complete octahedron, which is the object towards which the law of decrement tends.

But amid all these diversities of positions, the mutual incidences of the faces of the crystal are constant. This truth, which has been placed beyond all question by the numerous observations of Romé de l'Isle, is a necessary consequence of the integrant molecule being itself invariable in its form, and also from the law of decrement in its turn having a constant progress, which is only arrested more or less far from its limit in the different crystals relative to one and the same variety.

A second cause of variations is that which disturbs the symmetry and regularity of the crystalline form, and the effect of which is to destroy the equality of the analogous faces, in such a way that some take a very visible extension, while others almost entirely escape the eye. The theory ought to make an abstraction of these variations, and regard them as null: but they are visible enough to confuse the mind of a person not much habituated to these exercises, and who cannot easily distinguish the type from the true form through the traits which disfigure it, and this is the source of the greatest difficulties which the study of crystallography presents. The projections traced from regular crystals, and the copies in *relievo* of these bodies, may be of great use to the naturalist, in order to bring back the rest, by an exercise of imagination, to the symmetry from which they are separated.

These imitations of the work of nature will serve to obviate a difficulty of another kind, namely, that which arises from the grouping of crystals partly concealed by each other, or from their slight projection above the matrix,

trix, in which they seem to be more or less fastened, so that it behoves an observer to complete, in his imagination, each of these partial forms.

In short, I have been more than once surprised to see with what facility young mineralogists, who have joined to a taste for the science an aptitude for geometrical conceptions, have referred every thing to its right place in crystals the faces of which were the most deranged, or have profited from the trifling part of a crystal sunk in its matrix, in order to guess at the rest. It would even seem that there is a peculiar satisfaction attached to the solution of these small problems: every person is pleased with giving proofs of sagacity, and with understanding nature as if by half a word.

In order to determine the mutual incidences of the faces of a crystal, or of its salient angles, an instrument is used which was invented by M. Carangeau. This instrument, which strongly resembles the graphometer, is composed of a semicircle M T N (fig. 77), of brass or silver, divided into degrees, and which has two arms A B, F G, one of which F G is slit from *u* to R, excepting at K, where a small piece is left unslit in order to give more solidity to the instrument. This arm is attached at R and at *c* to a brass rule situated behind, and which is of a piece with the semicircle. The junction of the arm with this rule is produced by means of two screws which are inserted into the slit. The other arm A B is slit in the same manner from *x* to *c*, where it is attached above the former by means of the screw at this place, and which traverses the two slits. On loosening the screws, we can shorten at pleasure the parts *c* G, *c* B, of the two arms, as circumstances require.

The arm A B having only a single point of attaching at *c*, where the centre of the circle is, has a movement around this centre, while the arm G F remains constantly in the direction of the diameter which passes by the points zero and 180°.

It may be useful to remark, that the upper part of the arm A B ought to be bevilled off towards its edge *sz*, the direction of which being prolonged below, passes by the centre *c* of the instrument. The reason of this is, that this edge is what is called *the index line*, i. e. that which indicates on the graduated circumference the measurement of the angle wanted.

Let us now suppose that we wish to measure on a crystal the angle formed by two adjoining planes. We know that this

this angle is equal to that of two lines drawn from one and the same point of the edge which joins these planes, with the condition that they are perpendicular to this ridge and laid down on the same planes. In order to have this angle, we shall arrange the instrument so that the portions *c* G, *c* B of the two arms may leave no light between them and the planes in question, and at the same time their edges may be perpendicular to the edge of junction. In this case, the faces which embrace the crystal are tangents to the two planes whose incidence we seek for. This being done, we shall seek on the circumference of the instrument, the degree which the index line *sz* marks, or the angle which this line forms with that which passes by the centre *c* and by the zero point, which angle is equal to that formed by the two portions G *c*, *c* B of the arms, since it is opposite to it at the summit.

It is an advantage to be able to shorten these parts at pleasure, to avoid the obstacles which would render the operation impracticable, and which might be occasioned either by the matrix to which the crystal adheres, or from the adjoining crystals in which it is partly fastened.

But there are cases in which this precaution is not sufficient, and in which we should find ourselves constrained by the part of the semicircle situated towards M, if its position was invariable. The ingenious inventor of the instrument has guarded against this inconvenience by the following contrivance.

The stalk at *c* has, besides the two arms, a stay or rod of steel placed below the copper rule on which the arm G F is immediately applied. The upper extremity of this rod, or that which is situated towards O, has a hole into which a steel peg also enters, furnished with a screw in a similar manner. In addition to this the semicircle is divided at 90°, so that, by means of a hinge with which it is provided at the same place, the quarter of the circle T M is folded below the quarter of the circle T N, and is as it were suppressed. When we wish to execute this movement, we must loosen the screw which fastened the upper part of the rod *c* O, we must disengage the hole at the end of this rod from the screw which is inserted into it, and we must pull down the rod until it is beneath the copper rule which has the arm G F. When the angle measured exceeds 90°, we must return the quarter circle T M to its place, in order to ascertain its value.

It will be easy to appreciate the utility of the goniometer, if we reflect how interesting it is that descriptions of crystals

stals should indicate the angles which their faces make with each other. Such are the indications which make the description start up, as it were, by palpable and truly characteristic traces. Without these requisites, a description would be a rude and imperfect sketch, which might be referred to many different objects.

Thus we do not describe dodecahedral zircon when we merely say that it is a prism with four panes terminated by summits with four rhombs which arise on the longitudinal ridges. This character would also suit the harmotome (the cruciform hyacinth), the stilbite, oxidized tin, &c.: but if you add that the panes form right angles with each other, and the faces of the summit are inclined to each other by 124° 12′, the description will be restrained to zircon. If you say that the inclination is 121° 57′, it will be the harmotome; or, if you say that there are two different inclinations, the one 123° 32′, and the other 112° 14′, it will be the stilbite.

There are several varieties of one and the same substance which may present forms of the same kind, and which will only be distinguished by the measurements of their angles. Of this description are the six rhomboids on one hand, and on the other the two dodecahedrons with rhombic faces which are found in carbonated lime. How can we exactly describe all the varieties which differ from each other more or less, if we do not precisely mark the differences? And there are even cases in which the use of the goniometer is the only way to avoid an error which would not fail to slide into the description. Thus the calcareous rhomboid, the angles of which only differ in about 2° 18′ from the right angle, was at first taken for a cube, and would have continued to be called *cubical calcareous spar*, if geometrical measurements had not rectified this denomination, doubly defective, either in itself, or with reference to the theory which demonstrates that the existence of the cube does not agree here with that of any symmetrical laws of decrement.

One of the principal causes of this neglect of goniometry arises from the kind of rule to which some mineralogists are restricted, of confining themselves to characters susceptible of being determined solely by a reference to the senses: and on this account we are deprived of the resources presented by the instruments which give to our organs a new degree of delicacy, and render them capable of attaining, in the determination of the distinguishing characters of minerals, that precision which is in its turn the

the principal character of the sciences. I have known some admirers of simple and unaided ocular demonstration, nevertheless, approve of using an eye-glass. Now what is a goniometer but a kind of geometrical eye-glass, which enables us to perceive those minute differences, and imperceptible gradations, which escape the eye?

With respect to plane angles, we have sometimes indicated them also*, particularly those of primitive forms, and those which imprint on secondary forms a character of simplicity and regularity, such as the angles of 90°, 60°, &c.

We shall conclude from what precedes, that every crystalline form, when we consider only what is invariable in it, *i.e.* the number and the respective inclinations of its faces, is so truly characteristic, that it may serve of itself to determine, independently of every other consideration, the species to which the crystal belongs that presents it, provided it be not a cube, a regular octahedron, a regular tetrahedron, a rhomboidal dodecahedron, or a regular hexahedral prism. Thus the form of the dodecahedron with triangular scalene faces inclined among each other alternately by 144° 20′ 26″, and 104° 28′ 40″, indicates by itself a variety of carbonated lime.

Hence it would be possible to compose a method, by means of which, any crystalline form being given, we might succeed in ascertaining in what species it ought to be placed.

It is easy to perceive, that by considering the faces of crystals relative to their number which varies from four to 60 and upwards, with their vertical positions, inclined or horizontal, with the other modes of existence of which they are susceptible, we should have divisions and subdivisions so much the clearer, if geometry was called in to determine and circumscribe them. A method of this kind would be purely factitious, but it would fulfil its principal object: and we might even conceive that a geometrician with its assistance, who was no naturalist, and who had only before his eyes the collection of crystalline forms executed in wood, might succeed in arranging this collection. There would only be the forms common to various species which would lead to several names, among which we could only choose from the inspection of the natural crystals, by combining with the form a second

* We may measure these angles by means of a card properly cut, or by two very thin rules of steel, which turn on each other by means of a hinge.

character

character on which the last step would depend, by which to attain our object. Thus the taste joined to the cubical form would instantly indicate muriate of soda. A metallic colour of a bronze yellow reflected by a body of the same form would characterize sulphurated iron.

17. *Structure. Mechanical Division.* The character furnished by this operation is, as we have already remarked, the only one which does not participate in the variations produced by the mixture of heterogeneous substances, the influence of which modifies the hardness, specific gravity, fusibility, &c. and even the results of the analysis. It may perhaps disappear in the unshapen masses which have undergone a confused crystallization; but wherever it is possible merely to have a glimpse of it, it is susceptible neither of more nor of less. It removes in some measure every thing which is merely accessary in the composition of a substance; and while in all other respects this substance marches through a succession of shades, the measurement of the primitive angles stops at the same degree; and as soon as the substance changes its nature, there is an abrupt leap in the value of the angles.

We may venture to hope, that those who peruse this treatise with attention will perceive the advantage which we have made of the character in question, for the determination of the species. In our first researches we had nothing further in view than to make it the basis of a theory fitted for throwing lights on crystallography. But the various applications which we have made of this theory led us to exclude from such a species crystals which had been referred to it, and which rejected the laws of structure of which the forms relative to this species were susceptible; whereas other crystals, hitherto placed in different species, were subject to laws which solicited their intimacy; and from that moment we conceived that this theory, which at first appeared restricted to a simple branch of mineralogy, could extend its influence to the whole science, and contribute to give more regularity and justness in the distribution of the subjects which it embraces.

18. *Fracture.* This ought not to be confounded with structure. Having broken a mineral, when we perceive internally a scaly, granulous or fibrous texture, this is the effect of an arrangement which preexisted in the body. But if we find an undulated surface, or a species of small scales, which are nothing else than very thin fragments, still partly adhering to the substance, this aspect is the effect of fracture. But as it depends originally on a certain mode

mode of aggregation, it is generally discovered in all the pieces of the same substance, and it is this which may serve as a character for recognising it.

Minerals in which there is wanting some one of the sections necessary for completing the primitive form, present a fracture properly so called at the place where these sections ought to exist. For example, in the amphibole, the joints parallel to the panes of the prism are very distinct, whereas we perceive none in the direction of the bases; so that the crystal is broken, instead of allowing itself to be divided in the same direction. There are therefore, in these cases, longitudinal joints with a transverse fracture. In other cases the joints are parallel to the bases, and the fracture is longitudinal. We shall point out the different directions according to which the fracture takes place; and when there will be no joints visible in any direction, as takes place with respect to agate-quartz, we shall say that the fracture is indefinite.

[To be continued.]

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 31.—The conclusion of Mr. Home's paper on the organs of generation in ovi-viviparous animals, particularly the *squalus* or shark genus, and the opossum, was read. Mr. H.'s observations chiefly applied to the *squalus acanthias*, or picked dog-fish, common on the shores of Norfolk; and the kangaroo, particularly the latter, the young of which not deriving its nutriment by a navel-string, or from the uterus of its mother, is supported by external agents, of which air forms an essential part. He also noticed the fact, that fish deposit their eggs on rocks and plants near the surface of the water, which there contains more atmospheric air, and that this air is necessary to the life of the young fish, which are enveloped in a gelatinous fluid.

June 7,—was occupied in reading Mr. Brande's appendix to Mr. Home's paper, consisting of a chemical analysis of the peculiar gelatinous-like matter in which the ova of sharks, spawn of frogs, &c. are nourished. By the friendly assistance of Sir Joseph Banks, Mr. B. obtained some of what is called star-shot jelly from Lincolnshire (that substance found near marshes, which Mr. Pennant justly conjectured to be the excrement of herons after feeding on frogs); the matter which envelops frog-spawn, and that which

which includes the ova of sharks: these he respectively analysed, and found them to possess similar properties, but all very different from gelatin, however analogous in their external appearance, and which he concluded to be a peculiar animal matter not yet described. This jelly-like matter is insoluble by water, but it absorbs water in great quantities, and becomes proportionally enlarged in consequence: acids and alkalies, however, dissolve it; but in none of its characters does it evince any identity with gelatin or albumen.

A mathematical paper on multi-nomials, by Mr. Knight, was communicated to the secretary (Mr. Davy), and laid before the society; but it was not of a nature to be read.

Mr. Hubbard communicated a letter from Sir John ——, containing a plan for purifying the air of coal-mines. The author, having observed that workmen descend into wells with the greatest safety after throwing a quantity of water into them, proposes the like expedient to purify coal-mines, by projecting water, in quantities sufficient to absorb the choke-damp (carbonic acid gas), against the cieling of mines, by means of an instrument like a fire-engine, made with an end like that of a watering-pot, to throw the water like a shower-bath, and thus present the greatest possible surface to the noxious air. This machine, the writer concludes, might be both supplied with water and worked by the steam-engines in all coal-mines. Several other minute operations and less important advantages were stated as likely to result from the adoption of this plan; on concluding which, the society adjourned till

June 21,—when a part of a paper by M. Delille, translated from the French, was read, describing the *bohan upas*, or poison-tree, of Java. The author is a French physician, a member of the National Institute of Egypt, and transmitted this paper from the East Indies to the Royal Society, by means of an English lady. The botanical account of this poisonous plant he received from one of the French naturalists who accompanied Capt. Baudin, and who resided some time in Java; where he visited the interior of the country, and with much difficulty succeeded in prevailing on the natives to show him the different poison-plants, which they carefully conceal in order to use them during war. Hence the reason of so many fables as have been repeated respecting the extraordinary destructiveness and influence of the *upas*, which in the language of the Javanese signifies vegetable poison, and is applied only to the juice of the bohan tree, and another twisted-stemmed plant,

plant. The bohan is a large tree, which this writer considers a new genus: the other plant, yielding an equally powerful poison, is of the woodbine genus. The *upas*, or poisonous juice, is extracted by an incision in the bark with a knife, and carefully collected and preserved by the natives to be used in their wars. As to its diffusing noxious effluvia in the atmosphere, and destroying all vegetation around it, the absurdity of these stories is best exposed by the fact, that the climbing species requires the support of other plants to attain its usual growth. Dr. Delille made several experiments with the *upas* on dogs and cats. An incision was made in the thigh of a dog, and eight grains of *upas* dropped into it: shortly after the dog began to vomit, and continued vomiting at intervals, till he became convulsed, the muscles of his head greatly distorted, and he died in 20 minutes. Six grains were put into the thigh of another dog, which also vomited first his undigested food, next a white foam, and died contracted and convulsed in 15 minutes.—A cat was also treated in like manner; but she was still sooner and more convulsed, and her muscles contracted: she continued leaping up for a few minutes, and fell down dead. All these animals died crying and in great agony.—After repeating a number of experiments on the deleterious and prompt effects of this powerful poison when applied externally; the author gave a grain and a half to a dog, which he took into his stomach, but it only produced a slight purging. To another four grains were given, which in about four hours produced both vomiting and purging, and the dog died in the course of half a day. On examining the bodies of these animals after death, no very extraordinary appearances were discovered; the ventricles of the heart were full of blood, and some slight traces of inflammation appeared in the stomach; but the derangement was not so great as might have been expected from such a violent and sudden death. From this circumstance, the author concluded that the absorbents had transmitted the poison to the nerves of the stomach, and that this peculiar vegetable poison acts exclusively on the nerves.

*** In the account we gave (in our last number) of Mr. Macartney's paper on luminous animals (not insects merely) there were some mistakes. He stated that they belong to several classes, as *mollusca*, *insects*, *worms* and *zoophytes*.—There is but one species of mollusca luminous, the pholas dactylus. The medusæ that were ranked by Linnæus amongst mollusca are now placed more properly with zoophytes.—The medusa scintillans, one of the luminous

minous species, was not given to Mr. Macartney by Capt. Horsburg, but discovered by himself. The pyrosoma atlanticum of Peron was called, in our account, the pyrosoma *atlantica* of *Perot*.

A great part of Mr. Macartney's interesting paper is taken up with the anatomical description of the organs from whence the light issues in certain species.

ROYAL INSTITUTION.

In the concluding lecture at the Royal Institution, the large Voltaic apparatus, consisting of 2000 double plates of four inches square, was put into action for the first time. The effects of this combination, the largest that has ever been constructed, were, as might have been expected, of a very brilliant kind.

The spark, the light of which was so intense as to resemble that of the sun, struck through some lines of air, and produced a discharge through heated air of nearly three inches in length, and of a dazzling splendour. Several bodies which had not been fused before, were fused by this flame; the new metals discovered by Mr. Tennant, iridium, and the alloy of iridium and osmium. Zircon and alumine were likewise fused;—charcoal was made to evaporate, and plumbago appeared to fuse in vacuo. Charcoal was ignited to intense whiteness by it in oxymuriatic acid gas, and volatilized in it, but without effecting its decomposition. A large Leyden battery, containing 24 coated jars, was charged by a momentary contact of the wires to a degree that required from 20 to 30 turns of Nairne's electrical machine of eight inches diameter. All the electrical phænomena of the passage of electricity to a distance; the discharge through a Torricellian vacuum; the attractions and repulsions of light bodies, were demonstrated in a distinct way by means of this apparatus. It may be hoped that the application of so powerful an instrument, and such easy methods of producing the most intense heat, will lead to some new facts in analytical science.

WERNERIAN NATURAL HISTORY SOCIETY.

At a meeting of this Society, 19th May last, the Rev. John Fleming, Bressay, read an account of several rare animals found by him in Shetland, particularly Pleuronectes punctatus, a specimen of which he exhibited to the Society; Lucernaria quadricornis; Echinus miliaris, &c.; and some undescribed species, particularly a Flustra, which he proposed to call *Flustra Ellisii*, in honour of Mr. Ellis, the illustrator

illustrator of the corallines.—At the same meeting, Dr. John Barclay read remarks on some parts of the structure of the large marine animal cast ashore in Stronsay last year.

At a meeting on the 26th May, Dr. John Yule read a summary of experiments and observations on the germination of the Gramineæ, in which he stated some new facts respecting the œconomy of this useful class of plants, illustrated by a series of drawings and specimens of the germinated seeds of the Cerealia or cultivated species ; and of the buds of the stem, and panicle of viviparous Grasses.—And the secretary read a communication from William Fitton, esq. on the porcelain-earth of Cornwall.

FRENCH NATIONAL INSTITUTE.

[Concluded from p. 399.]

Messrs. Majendie and Delisle have communicated to the class their experiments made on animals by means of the matter with which the natives of the Isles of Java and of Borneo poison their arrows. [See ROYAL SOCIETY, two pages back.]

M. Vauquelin has also made some experiments of this kind : at the end of his chemical analysis of the juice of the belladonna, he speaks of the effects of this substance on animals. Those which he forced to swallow it, fell down as if intoxicated, in a delirium precisely similar to that produced by opium.

M. Sage has reported on the same subject some more experiments, which chance threw in his way, or which he collected from authors, and which confirm the action of this juice on the nervous system, and particularly on the brain.

A young practitioner in medicine, whose name has been mentioned in former annual reports, M. Nysten, has attempted to ascertain the effects of different gases injected into the blood-vessels of animals : he used the greater part of the gases with which we are acquainted. Atmospheric air, oxygen gas, the oxidulated azotic, carbonic acid, carbonic, phosphuretted and hydrogenated gases, &c. are in no respect deleterious. The oxy-muriatic, nitrous acid, and ammoniacal gases seem to act by very violently irritating the right auricle and the pulmonary ventricle. The sulphuretted hydrogen, oxide of azote, and azotic gases injure the contractile power of these parts : others also change the nature of the blood so completely, that respiration can no longer convert it from venous blood into arterial, &c.

MEDICINE

MEDICINE AND SURGERY.

M. Desessartz has communicated the history of an epidemic disease, which raged in three adjoining villages at the same time. Although generally depending on the inclemency of the weather, and on the bad quality of the fruits of the season, this epidemy presented a sensible variety in the nature and intensity of the symptoms, which necessarily gave rise to essential modifications in the treatment. He shows that the differences depended on the exposure peculiar to each of these villages, on the quality of their respective soils, productions, and way of life of the inhabitants.

M. Sage has presented to the class some reflections on the best means of remedying the bite of the viper, and a description of the effects of the poison of the tarantula, with the means employed in Spain for remedying it. All these remedies consist in the internal and external use of the volatile alkali.

M. Tenon continues to enrich the art of surgery. He has communicated to the class three memoirs, one on the exfoliation of the bones, another on the operation of trepanning the cranium, and a third on some kinds of hernia. In the first he inquires if the bones of the great extremities of the body are exfoliated after amputation; and it results from his numerous experiments on dogs, hares, and sheep, that after all amputations, the denuded extremity of the long bones is exfoliated, as also happens to the flat bones when laid bare, before being covered with a cicatrix. In the second he gives the description of all the phænomena which take place in the cure of a wound in the head, in consequence of which the trepan was resorted to, and which was cured after 151 days' treatment.

In the third, he describes an ingenious method resorted to by himself, for the reduction of two crural herniæ, and gives some observations on the operation for an inguinal hernia. In order to succeed in the reduction of these crural herniæ, "I directed an assistant to get upon the bed of the patient, and place himself between the knees of the latter, making him raise them as high as possible: the pillows being withdrawn, I employed another person to hold the leg and foot on the ruptured side, and to turn strongly inwards the great toe as well as the knee and thigh." When matters were thus arranged, M. Tenon succeeded by degrees in returning the intestines into the abdomen, so that the patient had no occasion for any operation.

 M. Pel-

M. Pelletan has communicated to us some interesting observations on aneurisms, and on the chirurgical operations which these diseases require.

M. Larrey has submitted to the class a memoir, on which a report has been made, and which suggests, in cases of gunshot wounds followed by gangrene, that we should not expect to put a stop to the gangrene by performing amputation.

AGRICULTURE AND RURAL ŒCONOMY.

M. Sylvestre, in the name of a committee, has made a report to the Institute on a work by M. Yvart, entitled, Method of improving Agriculture by Manures (*assolemens*). "The science of manuring," says the report, "has for its object to render any soil capable of yielding crops constantly in the most profitable manner, and without being deteriorated. This work, he adds, fulfils the important object which the author proposed, and merits the approbation of the class."

M. De Cubiere has read a memoir on the bald cypress (*cypress chauve*): it has for its object to enlighten agriculturists, to give them new ideas as to the vegetation of this fine tree, and to make them acquainted with all the advantages which they may expect from its culture. The report which has been made of this work, by our colleague, M. Mirbel, has obtained for M. De Cubiere the approbation of the class.

M. Leblanc, who spent several years in America, has communicated to us his views with respect to the facility of naturalizing the Vigonia sheep in the Alps and Pyrenees, and on the uses of their wool.

M. Poyfere-de-Ceré read a notice on the washing of the superfine wools in Spain, and on the great washing-house at Alfaro, near Segovia; a memoir, in which will be found an account of an expeditious, easy, and œconomical method of cleansing wools, and constructing washing stations.

Finally, our colleague, M. Percy, having collected in Spain some curious observations on the manufacture of the amphori and alcazaras used by the Spaniards for keeping their liquors cool, has communicated them, with the addition of some important reflections as to the utility of these vessels, and on the influence which they exercise on the liquids they contain.

LXX. *Notices*

LXX. *Notices respecting New Books.*

Mr. Leybourn, of the Royal Military College, has just published the tenth number of the Mathematical Repository, containing solutions to the mathematical questions proposed in the eighth number, and a series of new questions to be answered in a subsequent number; an essay on polygonal numbers; a new demonstration of the binomial theorem; an illustration of the forty-seventh proposition of the second book of the Principia; a curious indeterminate problem; solutions to a curious problem in dynamics; and a continuation of Le Gendre's memoir on elliptic transcendentals.

Mr. W. Moore, of the Royal Military Academy, has in a good state of forwardness, A Treatise on the doctrine of fluxions; with its application to all the most useful parts of the true theory of gunnery, and other very important matters relating to military and naval science. The fluxions will be preceded by such parts of the science of mechanics as are necessary for reading the work without referring to other authors; and the whole will be so arranged, that any person moderately skilled in algebra, geometry, and trigonometry, and having a knowledge of the most common properties of the conic sections, may proceed to these inquiries with every interest and success. The whole will be printed in one volume octavo, and will be particularly adapted to all military institutions of eminence.

Mr. Marrat, of Boston, Lincolnshire, has in the press a work on mechanics, which is principally intended for the use of schools, &c.

The author's principal aim in composing this work, has been to make the subject easy to be understood by students, to facilitate the business of instruction, and to condense as much useful matter as possible into a small compass. In order also to blend theory with practice, and to remove the irksomeness which students mostly complain of in studying the theory, a great number of examples will be given in almost every section; these will be found of service in fixing the principles in the mind, and cannot fail of rendering the subject more easily attainable, and, consequently, of creating a stimulus to further exertions.

The work will be divided into five books; the first of which treats of statics, the second of dynamics, the third

of

of hydrostatics, and the fourth of pneumatics. In these four books the subjects are prosecuted as far as could be done without introducing the fluxional calculus; but to render the work of more general utility, and to accommodate students in the higher classes, a fifth book is added, in which several branches in the preceding books are very much extended. The work will be comprised in one octavo volume, and will be accompanied by thirteen copper-plates.

M. VIBORG, professor in the Royal Veterinary School, in Sweden, has published a dissertation "On the use of the flesh of horses."—The publication of this paper has had the effect of introducing the use of this article as food throughout Sweden, and the butchers' shops are now supplied with the carcases of horses, in addition to those of oxen. M. Viborg assures his readers, that the flesh of those animals, when roasted, is preferable to that of oxen.

LXXI. *Intelligence and Miscellaneous Articles.*

DE LUC'S ELECTRIC COLUMN.

THE small bells noticed in our last three numbers, still continued to ring on the 25th instant (June), as they had done since the 25th of March, without being known to have once ceased ringing. We are happy to be enabled to add, that those who wish to possess electric columns, fitted up in the form of rods, as described in our number for March last, may obtain them of Mr. Blunt, optician, Cornhill.

The success of the several charitable institutions for the relief of the indigent blind, has suggested the humane idea of bringing forward, for the equal relief of their opulent brethren in this country, a plan, similar to that by which M. Haüy, in Paris, taught them, several years ago, reading, writing, arithmetic, music, and the rudiments of the sciences generally.

LOCUSTS.

Rome, 29th May, 1810.

For some days past crowds of people, excited by curiosity, have been thronging the banks of the Tiber to witness a singular phænomenon. A wind from Africa has brought into these countries an immense swarm of locusts. These insects, having wasted the country, and now unable to find subsistence, have waged war among themselves, and devour each other. The weaker party take flight, and, pursued

sued by the vanquishers, throw themselves in myriads into the Tiber. The day before yesterday this river was covered with them.

Report to the Committee of the Honourable House of Commons, on the Petition of the Trustees of the British Museum; respecting the Purchase of Mr. Greville's Collection of Minerals.

London, May 9, 1810.

We the undersigned, having been requested by the committee of the honourable house of commons, on the petition of the trustees of the British Museum, to make a careful examination of the collection of minerals belonging to the right honourable Charles F. Greville, and to put a value upon the same with as much fairness and accuracy as possible;—have now to report:

First.—That on the 2d of this month we assembled at the house of the late Mr. Greville on Paddington Green, commenced our inspection of the collection of minerals; and continued the same, day after day, up to the 9th instant.

Second.—That we have found the specimens scientifically arranged, for the greater part, in glazed drawers, which are contained in cabinets made of beautiful mahogany.

Third.—That, exclusive of these cabinets, there are two others, containing models in wood and in clay, the former having been most accurately made by the Count de Bournon for the late Mr. Greville, exemplifying and elucidating the various figures and modifications of crystallized mineral substances; a series of great importance to mineralogical science.

Fourth.—That, in addition to the minerals contained in the drawers, there are arranged on the upper part of the cabinets many large and magnificent specimens, several of which are uncommonly rare and highly valuable.

Fifth.—That the whole collection consists of about 20,000 specimens.

Sixth.—That the specimens in general throughout the collection appear to us to have been selected with very great judgement, both as to their utility and beauty.

Seventh.—That the series of crystallized rubies, sapphires, emeralds, topazes, rubellites, diamonds, and precious stones in general, as well as the series of the various ores, far surpass any that are known to us in the different European collections.

 Eighth,

Eighth.—That we consider the entire collection to be equal in most, and in many parts superior, to any other similar collection which any of us have had opportunities of viewing in this and other countries.

Ninth.—That having accurately examined and separately valued the different cabinets and detached specimens, we find the total amount to be *thirteen thousand seven hundred and twenty-seven pounds.* £13,727.

WM. BABINGTON.	ROBERT FERGUSON.
L. Comte De BOURNON.	CHARLES HATCHETT.
RICHARD CHENEVIX.	WM. H. WOLLASTON.
HUMPHRY DAVY.	

We whose names are underwritten, and who have signed the foregoing report, think it but an act of justice on our part, to request permission to state to the committee the very great services which have been rendered by the Count de Bournon, during the whole of the inspection and valuation of the collection, with which he alone was well acquainted, having principally contributed to form it, and having been occupied for several years in arranging it for the late Mr. Greville. Without the able assistance of the Count de Bournon, so justly celebrated for his profound knowledge in mineralogy, the inspection and valuation would have required a very great length of time, and after all would most probably have been less accurately performed.

We therefore unanimously concur in giving this public testimony to the merits and services of Count de Bournon.

We also have to make our acknowledgments to Mr. Lowry and Mr. Jonville, who obligingly attended the inspection at our request, and rendered us much valuable assistance.

WM. BABINGTON.	ROBERT FERGUSON.
RICHARD CHENEVIX.	CHARLES HATCHETT.
HUMPHREY DAVY.	WM. H. WOLLASTON.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Halliday, master of the band belonging to the Cavan regiment of militia, for certain improvements in the musical instrument called the bugle horn.—May 5, 1810.

To William Chapman, of the town and county of Newcastle-upon-Tyne, civil engineer, for a wheel or wheels to be moved by water, steam, or any other suitable fluids or gases, and to be applicable to mechanic or other purposes where a moving force is required.—May 9.

To John Bosworth, of Birmingham, in the county of Warwick,

Warwick, coal dealer, for improvements in carriages to facilitate the unloading of heavy coals and other things.—May 9.

To Sir Issac Coffin, bart. vice admiral of the blue squadron, who, in consequence of a communication to him by a certain person residing abroad, is become possessed of a new invention of a perpetual oven for the making of all kinds of bread, a patent for the same, dated the 15th of May.

To James Bell, of Fieldgate-street, Whitechapel, sugar-refiner, for his certain improvements in the manner of refining sugar, and of forming sugar loaves of a particular description.—May 17.

To Charles Stewart, of the parish of St. Martin in the Fields, in the county of Middlesex, cabinet-maker, for certain improvements in the construction of dining- and other tables.—May 22.

To John Onions, of Broseley, in the county of Salop, iron-master, for his machine for thrashing corn and other grain, on a new construction.—May 22.

To William Docksey, of the city and county of Bristol, millwright, for improvements in the process of manufacturing an article commonly called ivory black, and for pulverizing, grinding, or reducing to a subtle and fine powder, all articles capable of a more easy separation of their parts or constituent principles by torrefaction, heating, or calcination in open or close kilns, ovens or furnaces, especially potter's clays, flints, colouring and glazing materials.—May 22.

To Joseph Anthony Berrollas, of Cowper's-row, Clerkenwell, in the county of Middlesex, watchmaker, for his warning watch on a new construction.—May 26.

To George Hickford, of Chadacre-Hall-Farm, in the parish of Strumpling, in the county of Suffolk, farmer, for his improvements upon the plough heretofore used for draining land, and the machine for drawing the same through the ground, whereby a horse will be able to perform a much greater quantity of work in the same time than by the methods now in use.—June 8.

To John Williams, of Cornhill, in the city of London, stationer, for certain apparatus or additional parts to be applied to and used with wheel carriages in order to render the same more safe and commodious.—June 8.

To Mary Townley, of Ramsgate, in the county of Kent, for the prevention or cure of smoky chimneys.—June 8.

To Arthur Woolf, of Lambeth, in the county of Surry, engineer, for certain improvements in the construction and working

working of steam engines, calculated to lessen the consumption of fuel.—June 9.

To Joseph Warren Revere, of Boston, in the United States of America, at present residing in London, for a new and improved method of splitting hides and shaving leather.—June 19.

To Joseph Clisild Daniell, of Frome, in the county of Somerset, clothier, for certain improvements on machines called gigs and shearing frames, used for dressing cloths, and in the clothing manufactory.—June 19.

To Malcolm M'Gregor, of Bell Yard, Carey-street, in the county of Middlesex, musical instrument maker, for certain flute or musical wind instruments with improved keys, which keys are also applicable to flutes and various other wind instruments now in use.—June 19.

To George Adams, of the Woodlands, in the parish of Lindridge, in the county of Worcester, farmer, for an improved method of cultivating of land, and of feeding and consuming the produce thereof by cattle and sheep, and of preserving and applying the manure of and made by such cattle and sheep, by means of certain houses made and built for the protection of cattle and sheep from weather, and feeding the same thereon, and moveable by means of wheels, slides, iron railways, or otherwise, invented and found out by him.—June 19.

To John Lindsey, (late lieut.-col. of the 71st regt.) of Grove House, in the county of Middlesex, for his boat and various apparatus, whereby heavy burthens can be conveyed on shallow water on rivers wherein shoals and other difficulties impede navigation, whereby the lives of men will be saved from wrecks and other situations of imminent danger at sea or on rivers, whereby the apparatus above specified may, in its consequences and constructions, embrace other important results highly beneficial to the British navy and commerce, by enabling the bottoms of ships to be examined with accuracy and expedition without the necessity of moving the masts or cargo.—June 19.

To William Bell, of Handsworth, near Birmingham, in the county of Warwick, engineer, for his improved machine for the purpose of cutting pasteboard, or cards out of pasteboard or paper, and for cutting various other articles.—June 19.

To James Frost, of Little Sutton-street, Clerkenwell, brass-founder, and James Frost the younger, his son, also brass-founder, for improvements upon cocks, or an improved lock cock.—June 22.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For June 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May 27	47	60°	46°	29·95	40	Fair
28	48	56	45	30·13	53	Fair
29	46	51	46	·40	71	Fair
30	49	65	51	·30	66	Fair
31	52	66	50	·30	60	Fair
1	53	65	52	·29	77	Fair
2	54	69	54	·25	79	Fair
3	53	66	50	·22	63	Fair
4	52	65	55	·20	90	Fair
5	54	55	50	·24	41	Cloudy
6	51	70	55	·19	79	Fair
7	54	67	56	·11	55	Fair
8	56	70	60	·05	76	Fair
9	59	73	55	29·94	52	Fair
10	59	70	55	·72	41	Cloudy
11	58	68	54	·82	53	Fair
12	54	66	53	·94	59	Cloudy
13	55	60	50	·85	0	Rain
14	50	63	54	30·15	70	Fair
15	54	66	51	·19	79	Fair
16	52	62	46	·03	55	Fair
17	50	60	54	29·95	51	Fair
18	56	70	60	·95	65	Fair
19	60	70	60	·95	51	Cloudy
20	63	72	63	·95	42	Cloudy
21	64	77	66	30·20	92	Fair
22	66	73	55	·30	55	Cloudy
23	54	69	55	·38	57	Fair
24	56	73	60	·25	61	Fair
25	59	73	66	·09	77	Fair
26	59	61	55	·05	30	Cloudy

N. B. The Barometer's height is taken at one o'clock.

ERRATUM.

In Mr. Davy's article (*see page* 409) for Plates IX and X, *read* Plates XII and XIII.

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Printed by Richard Taylor and Co. Shoe Lane, London.

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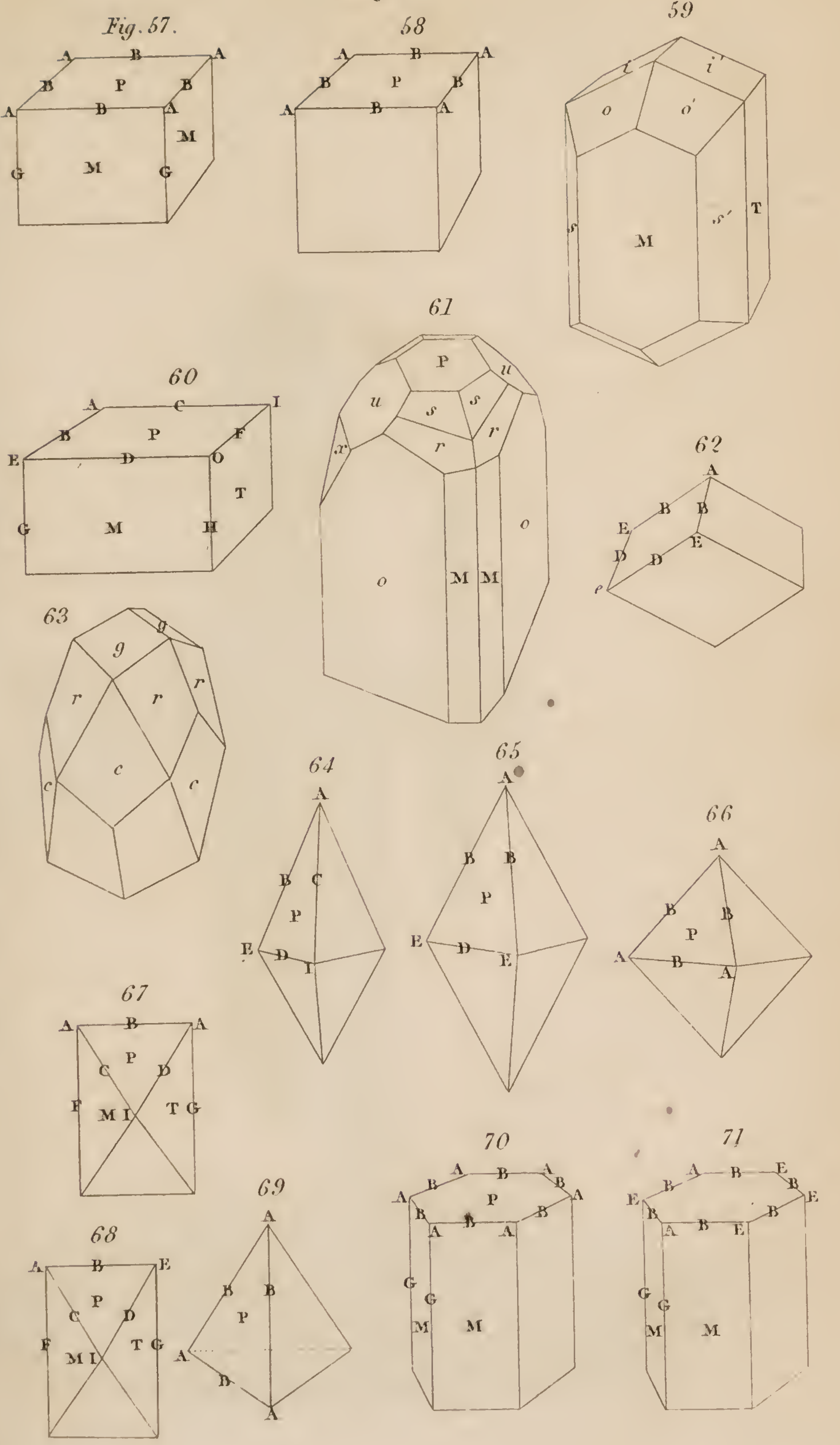
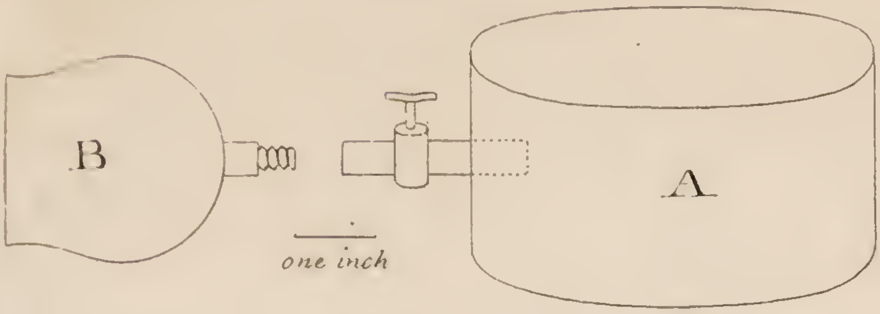
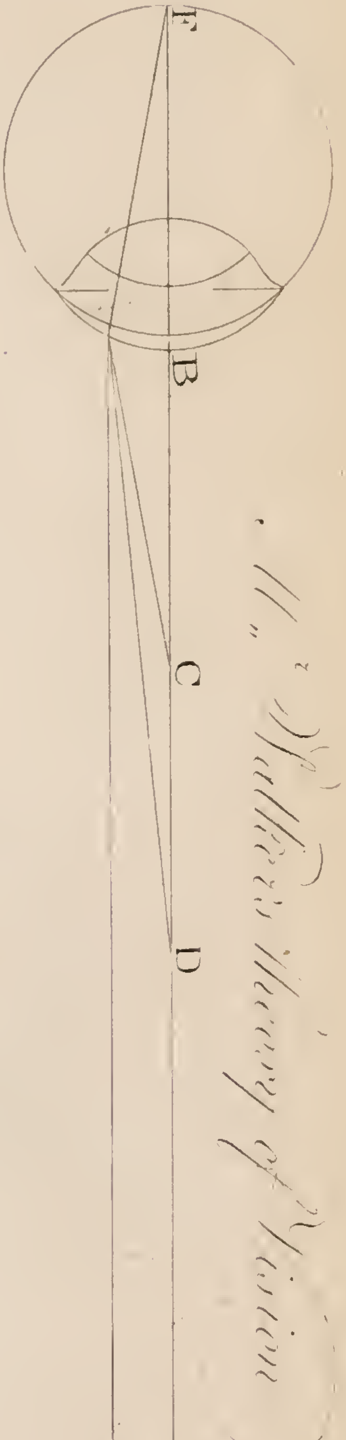
Haüy Pl. 7.



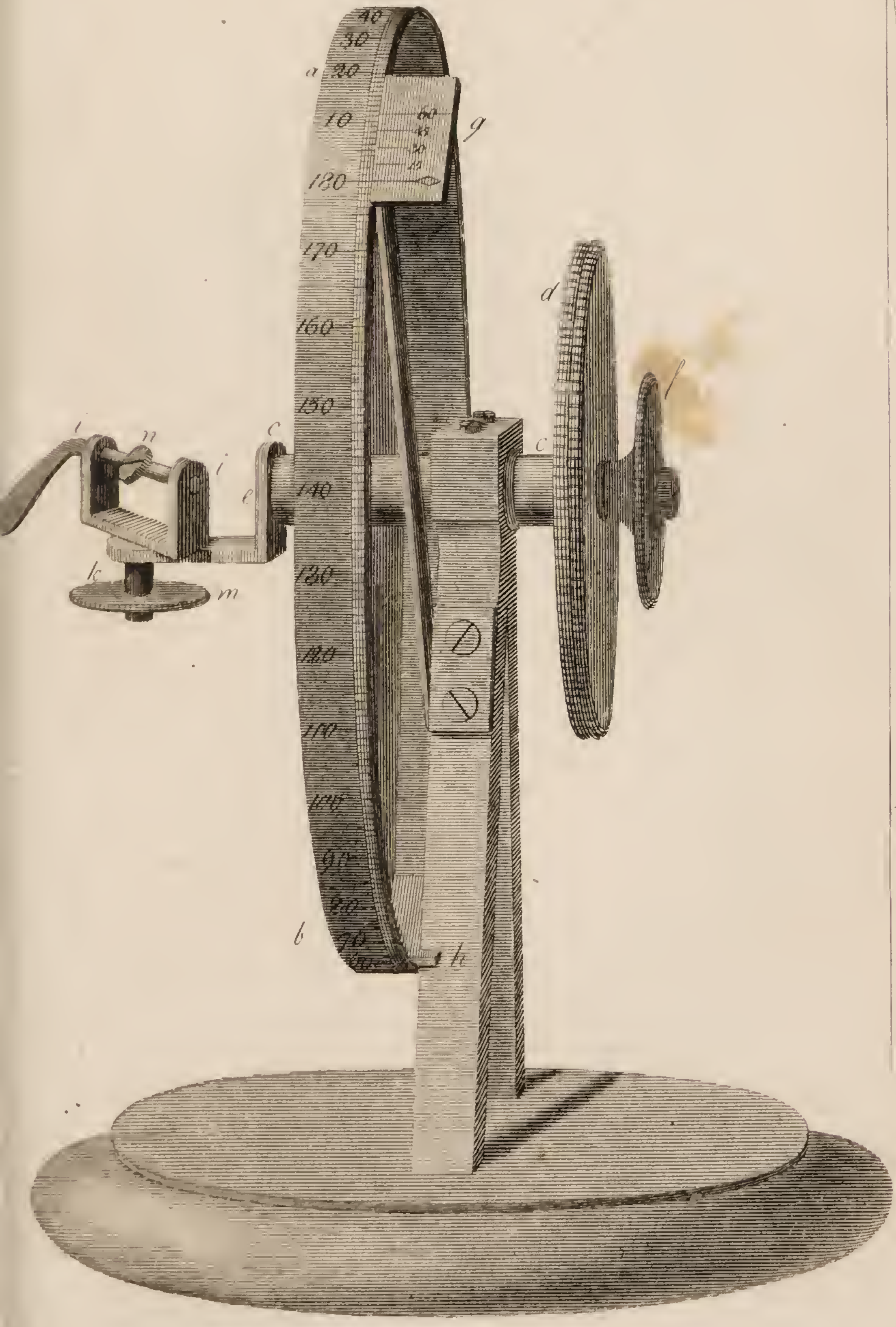
Lowry Sc.

Dr. Healy's cupping instrument





Mr. Walker's theory of Vision



S. Porter sc.

Haüy Pl. 8.

Fig. 77.

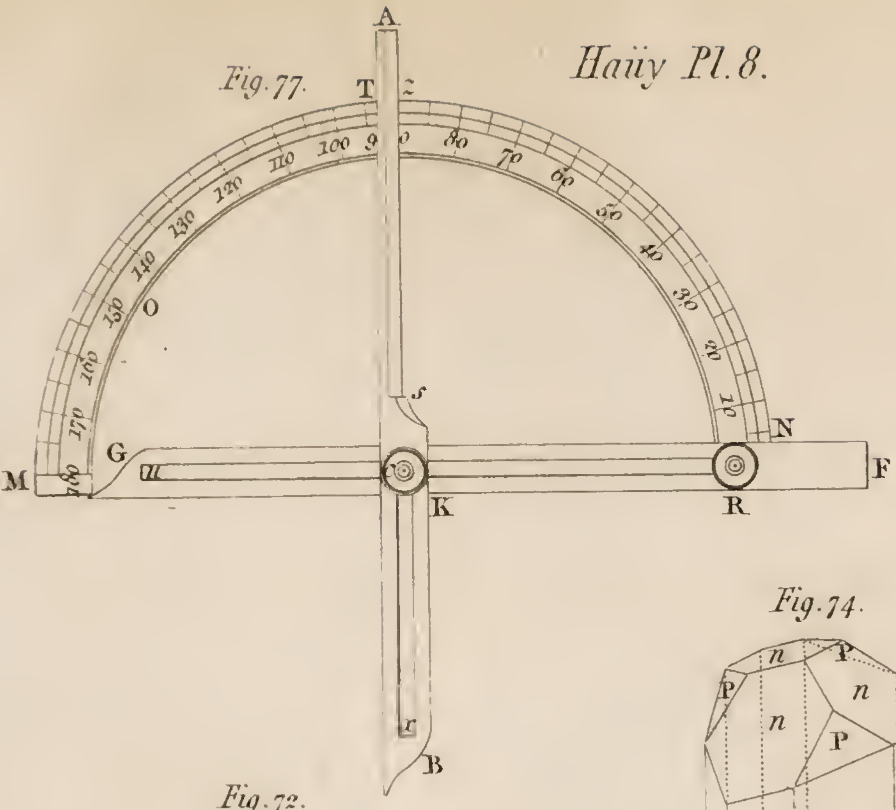
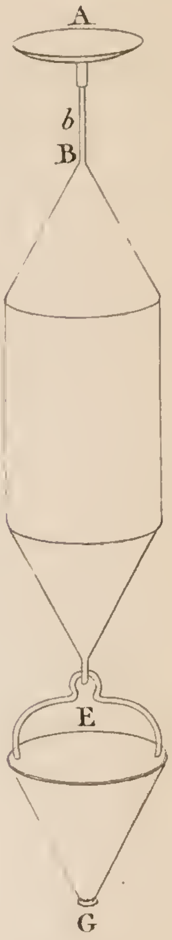


Fig. 75.



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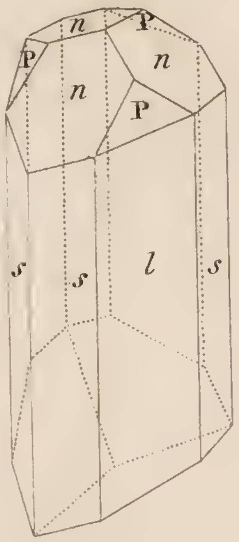


Fig. 72.

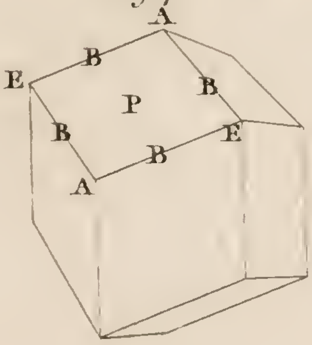


Fig. 73.

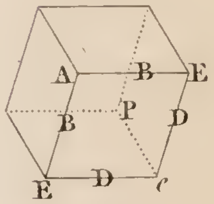
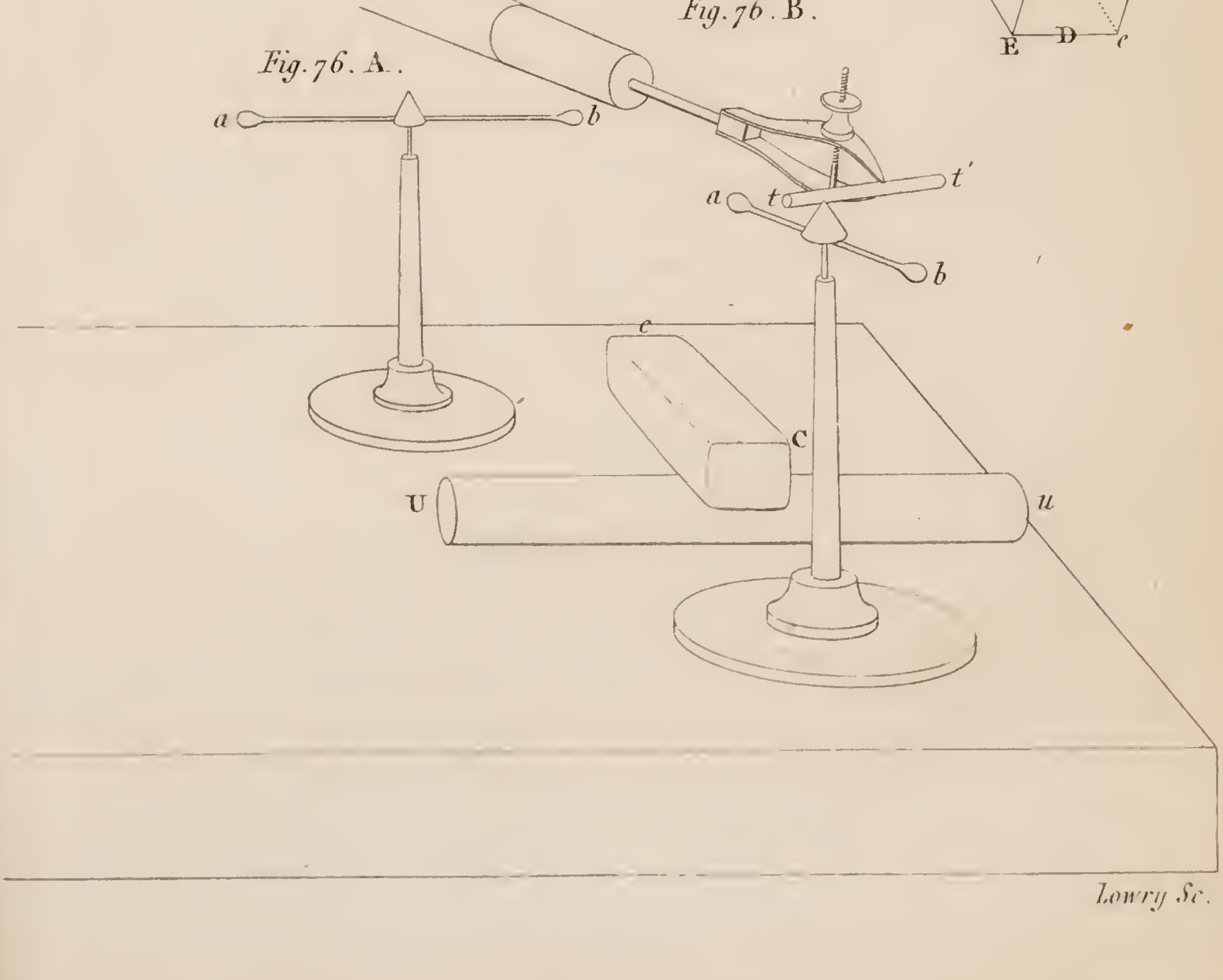


Fig. 76. A.

Fig. 76. B.



Lowry Sc.

Haüy Pl 9.

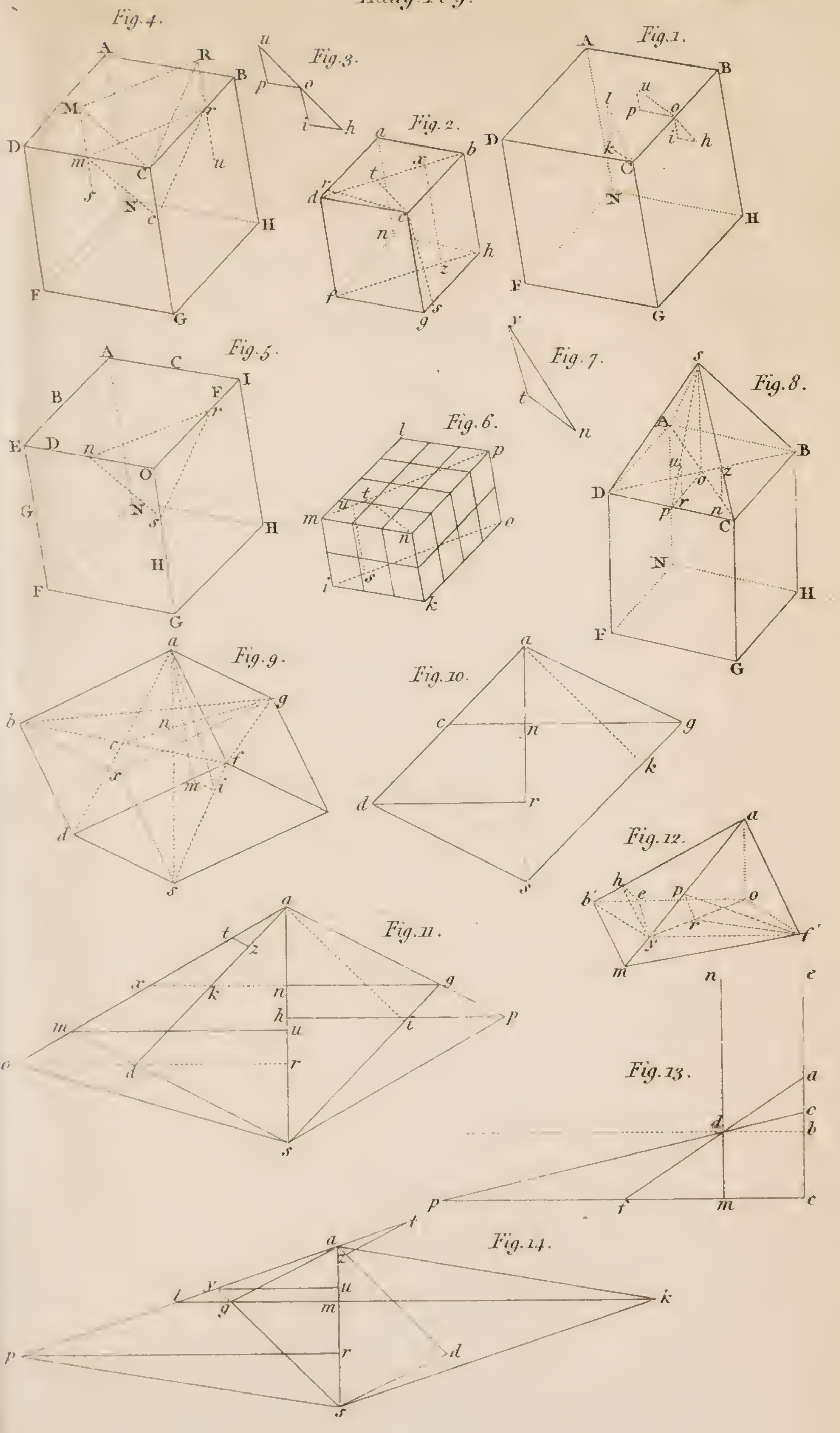

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Fig.3.
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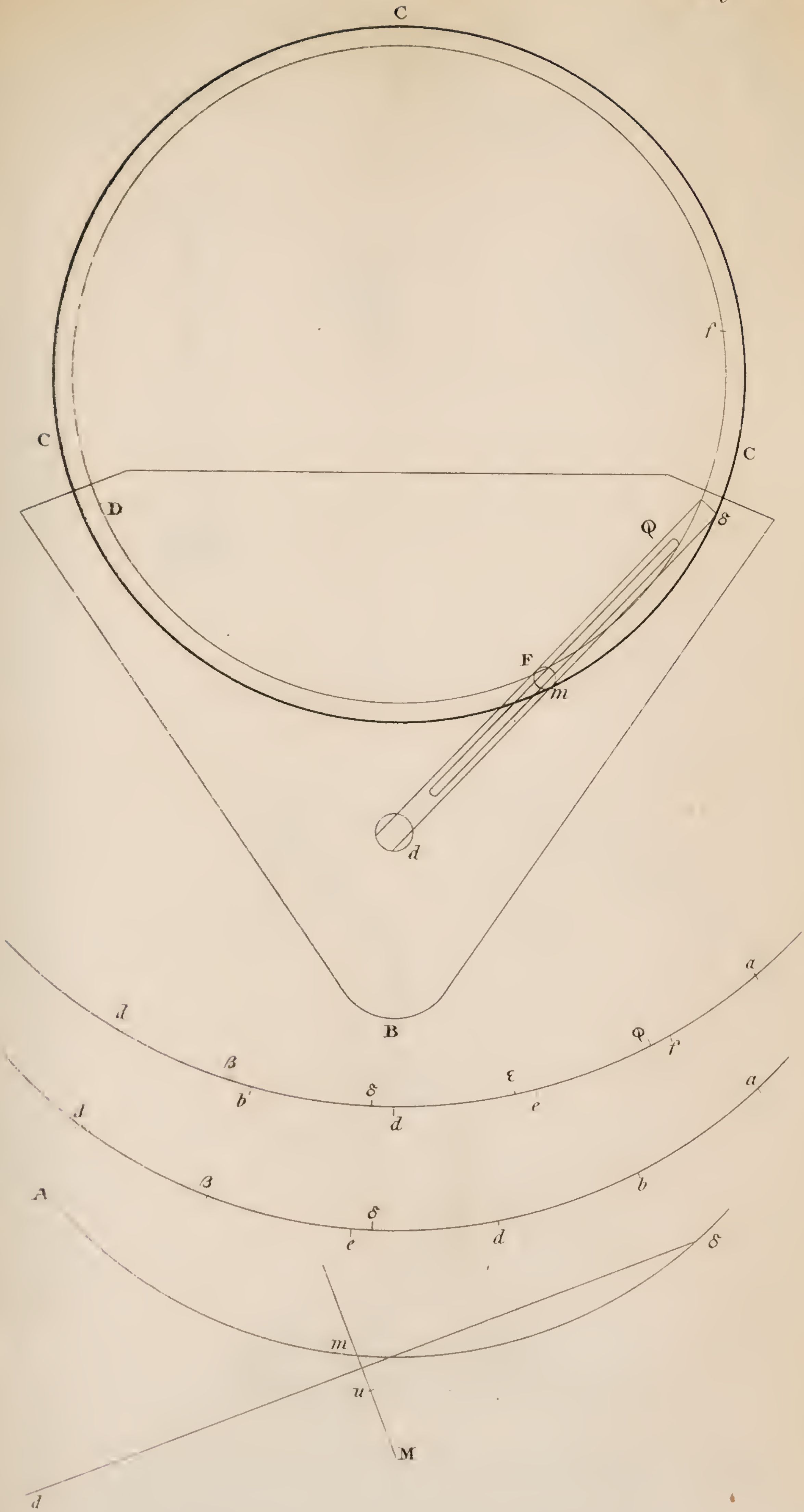
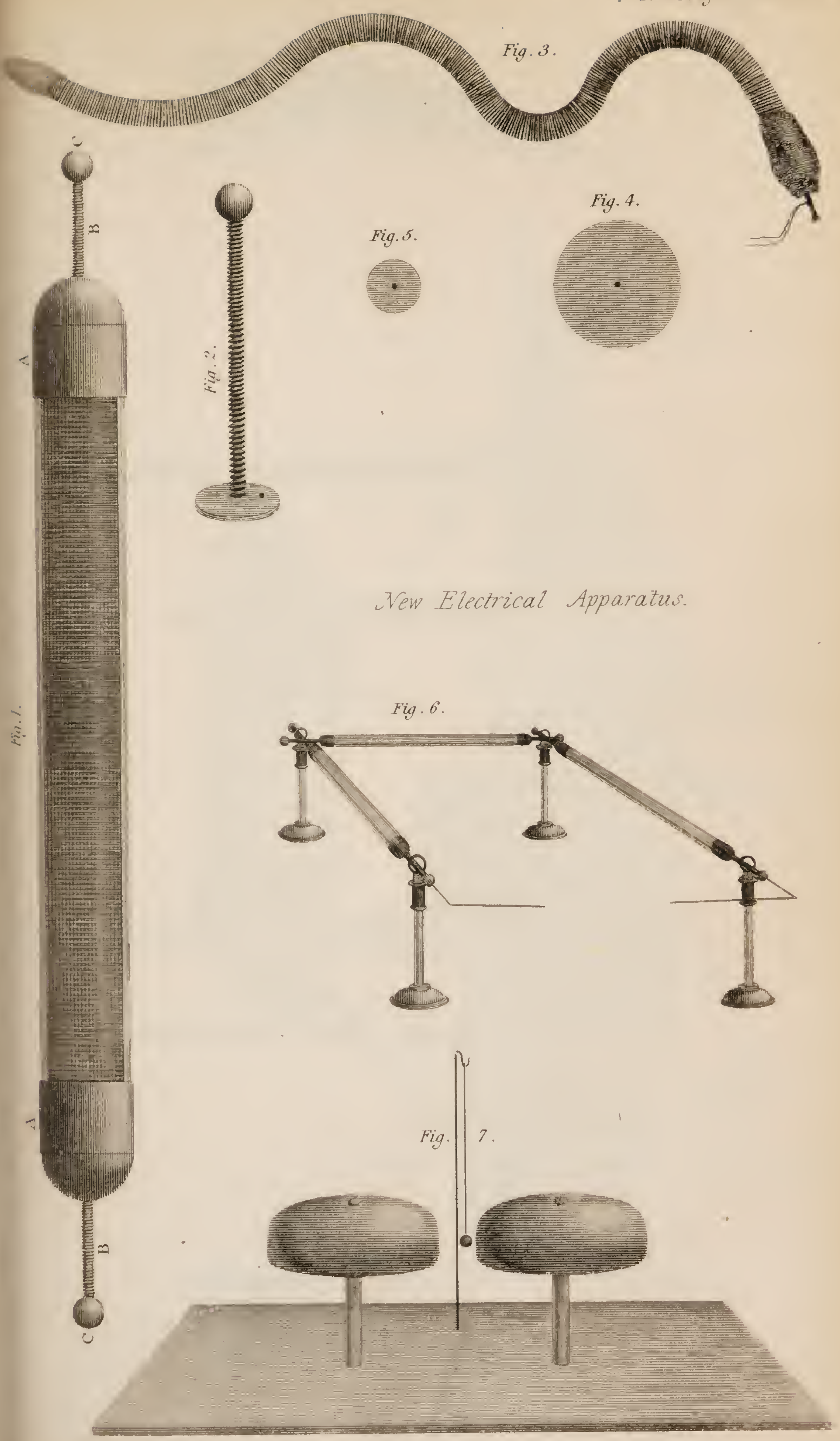


Mr. Cavendish's Dividing Instrument.



New Electrical Apparatus.

Accum's Hydro-pneumatic Table.

First Appearance.

Fig. 1.

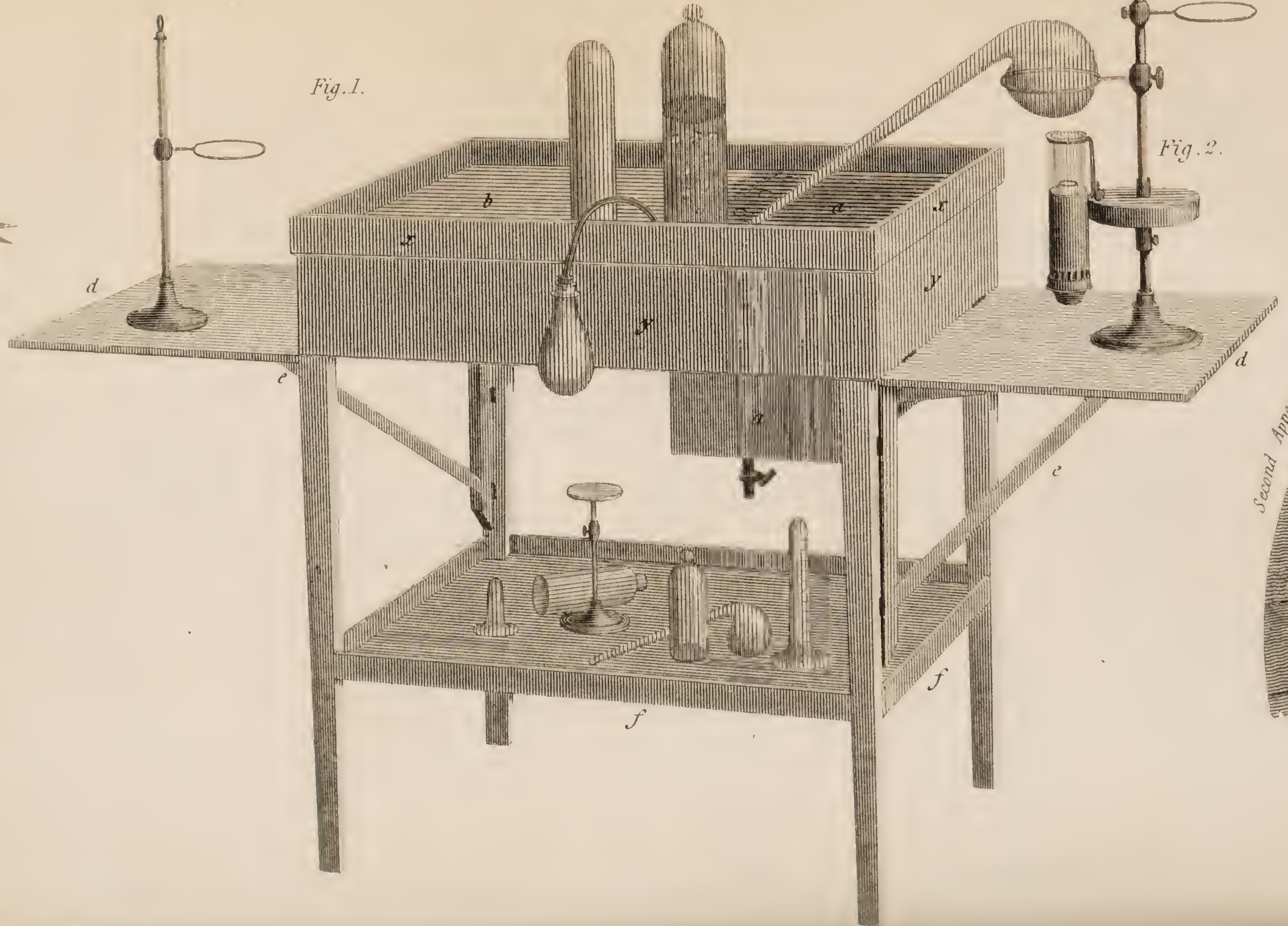


Fig. 2.

Second Appearance.

Haüy Pl. 10.

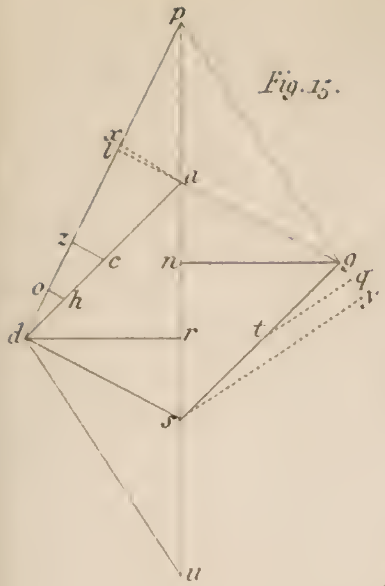


Fig. 15.

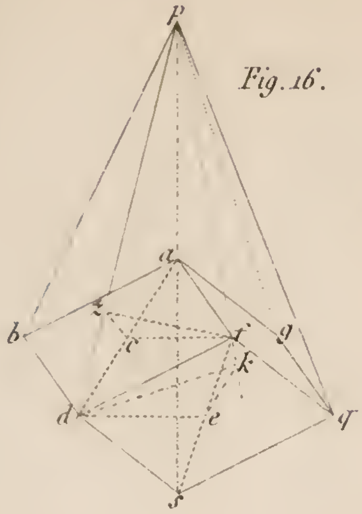
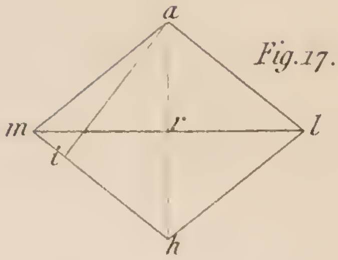


Fig. 16.



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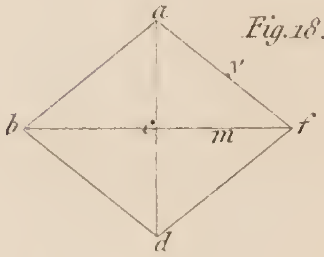


Fig. 18.

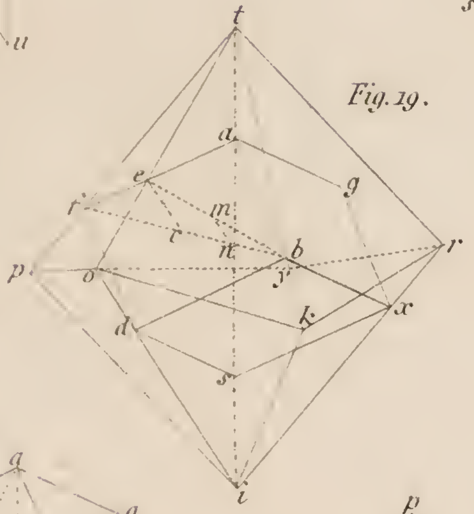


Fig. 19.

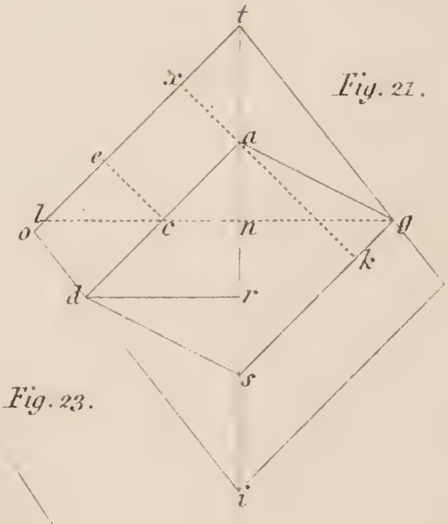


Fig. 21.

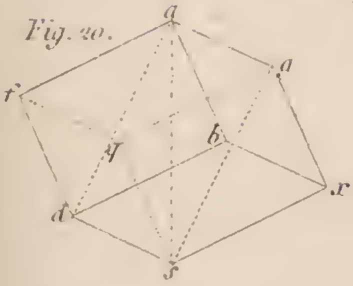


Fig. 20.

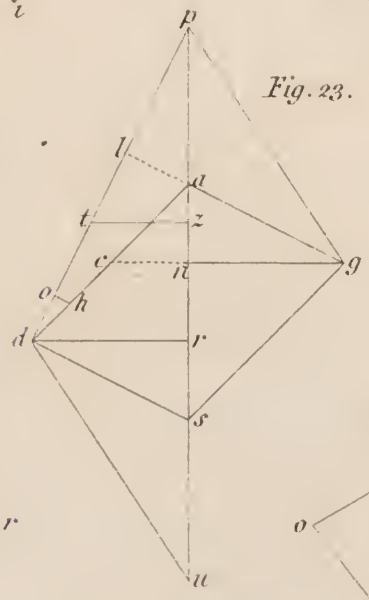


Fig. 23.

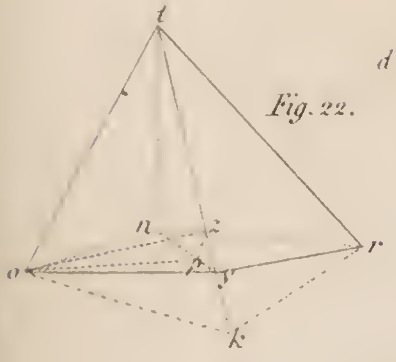


Fig. 22.

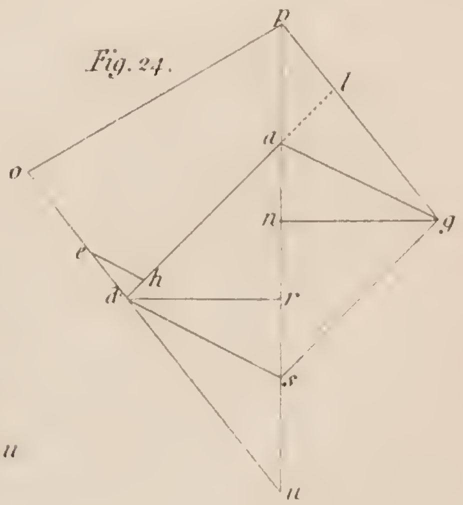


Fig. 24.

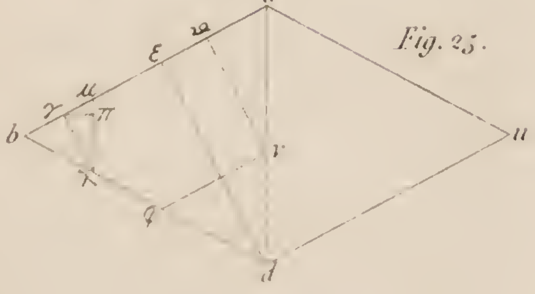
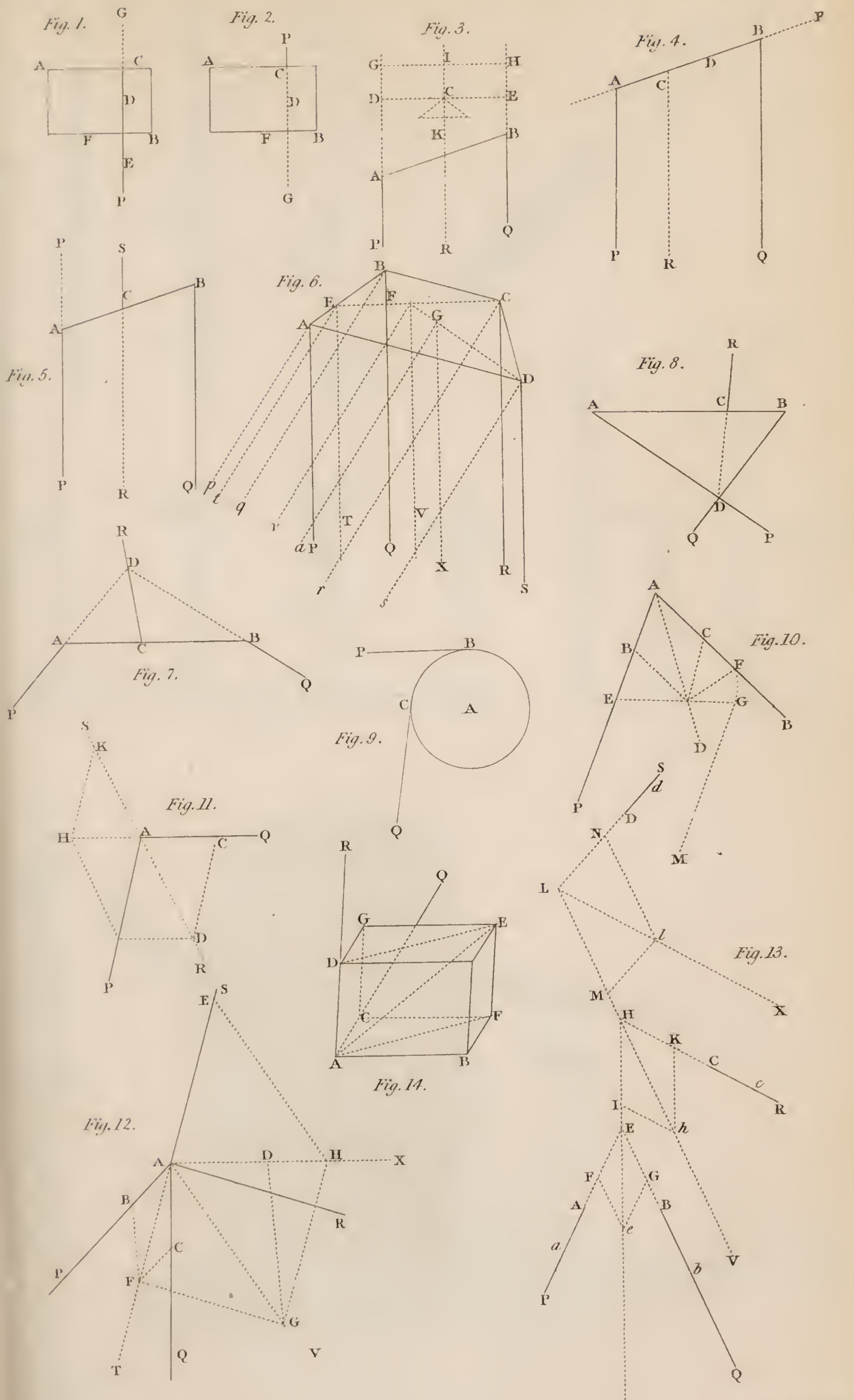


Fig. 25.

Lowry Sc.

Composition & Decomposition of Forces.



Lowry sculp.

Capt. Pasley's Telegraph.

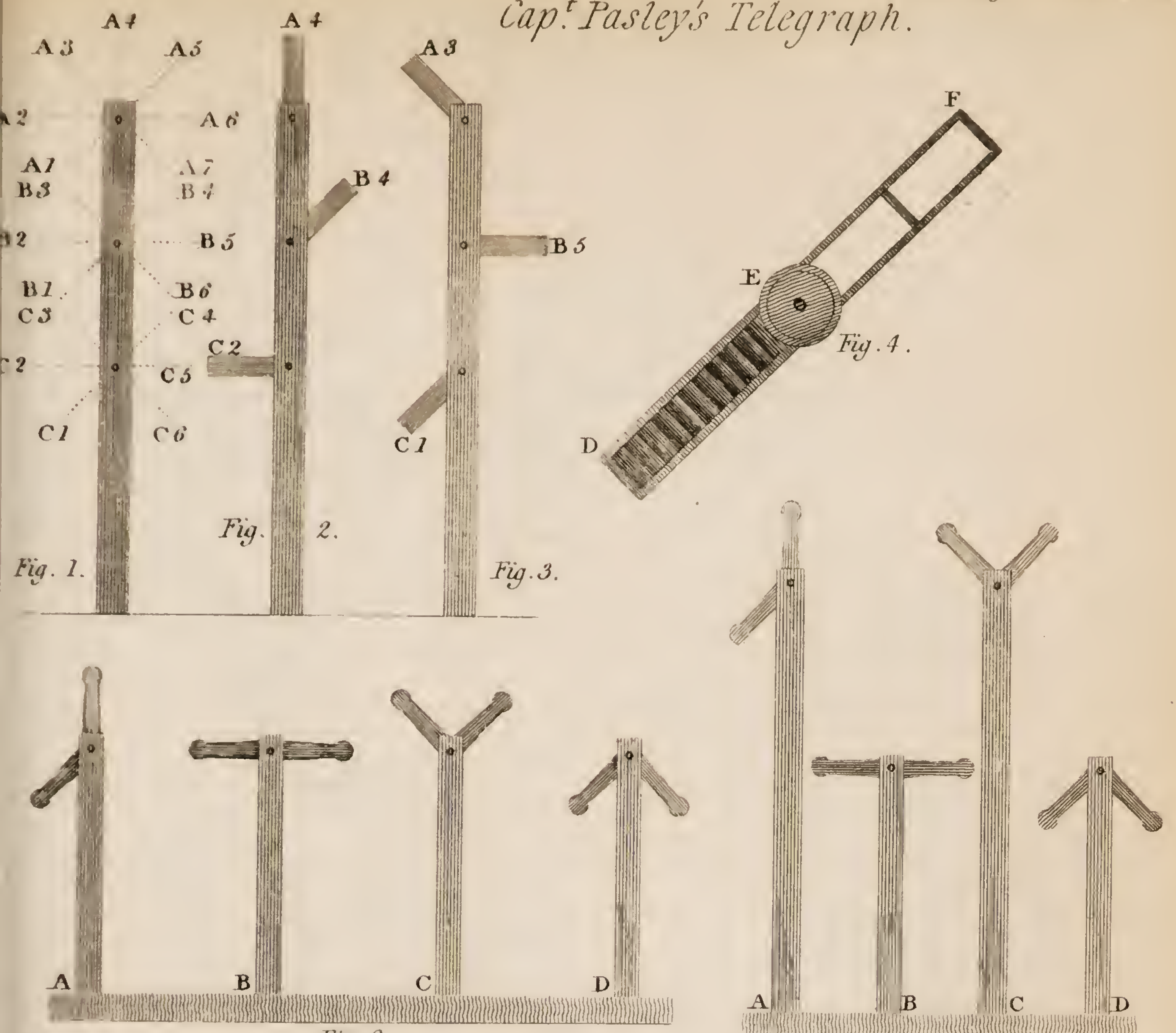


Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 6. Fig. 7.

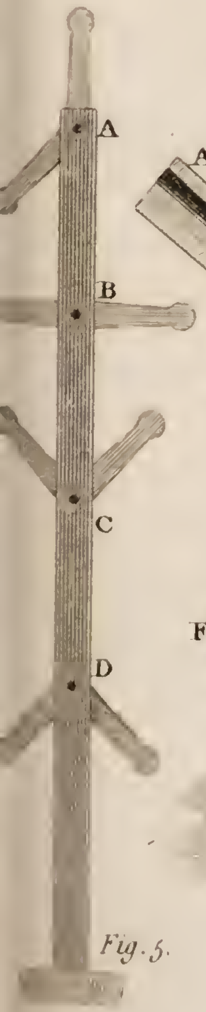
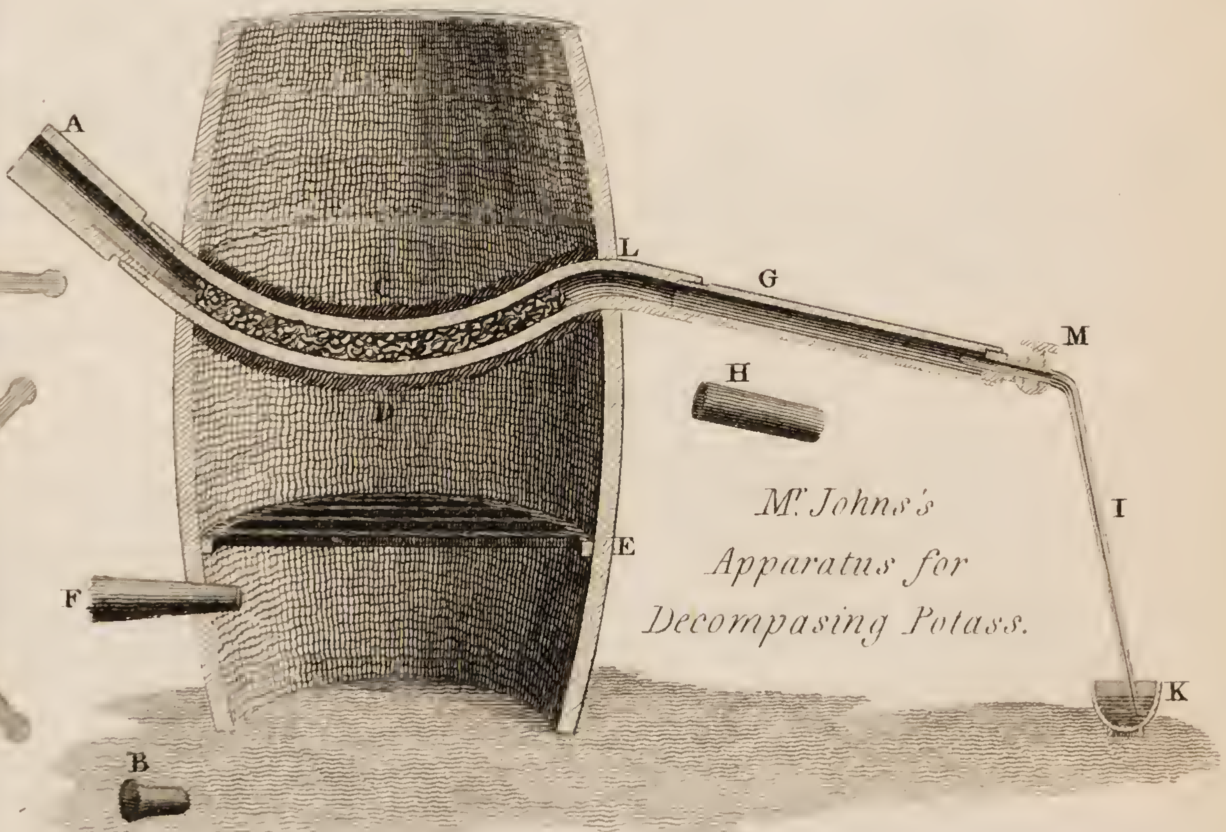


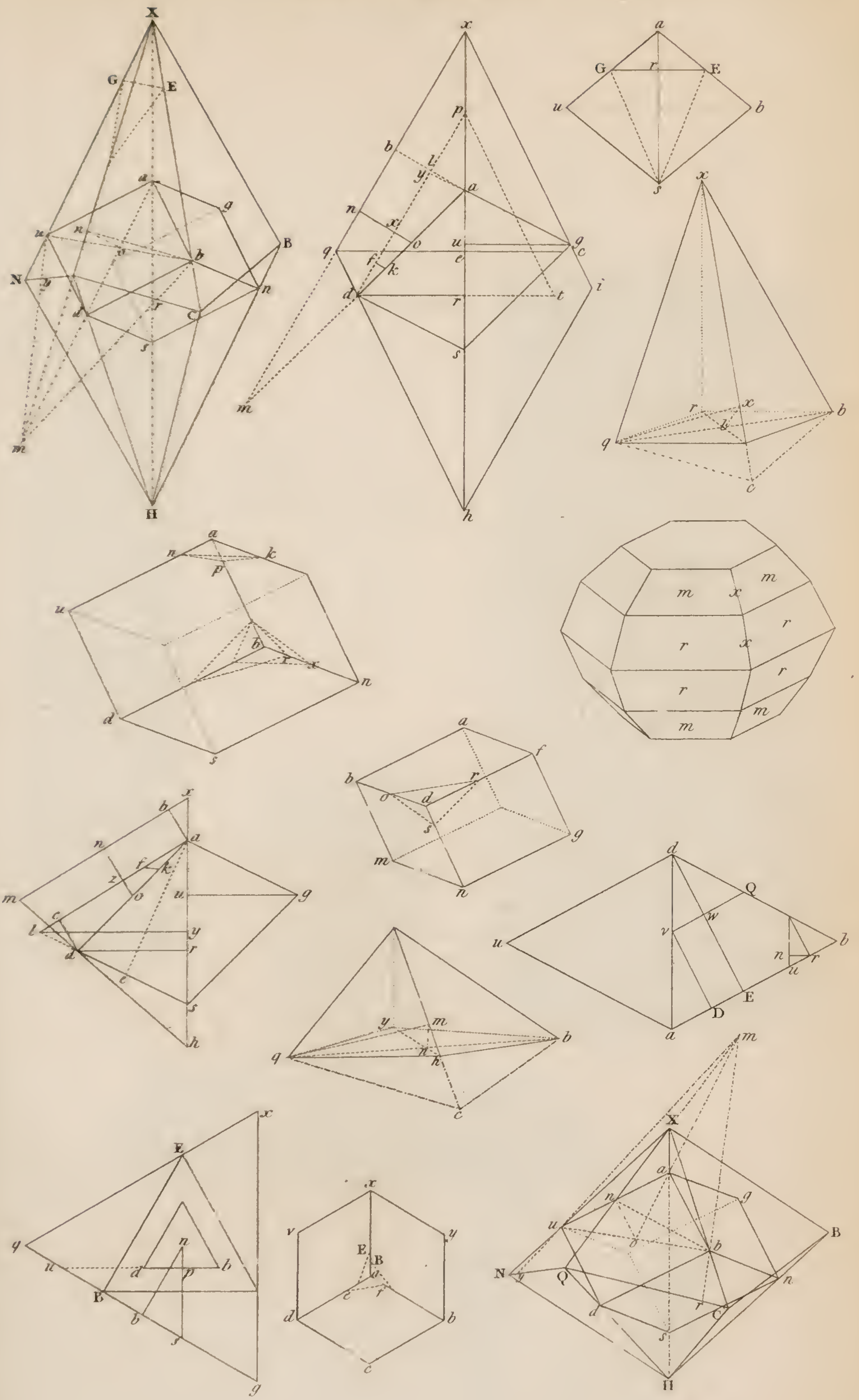
Fig. 5.



Mr. Johns's Apparatus for Decompasing Potass.

S. Porter, sc.

HAÜY PLATE II.



Lowry sculp.

Professor Davy's Electro-chemical Apparatus.

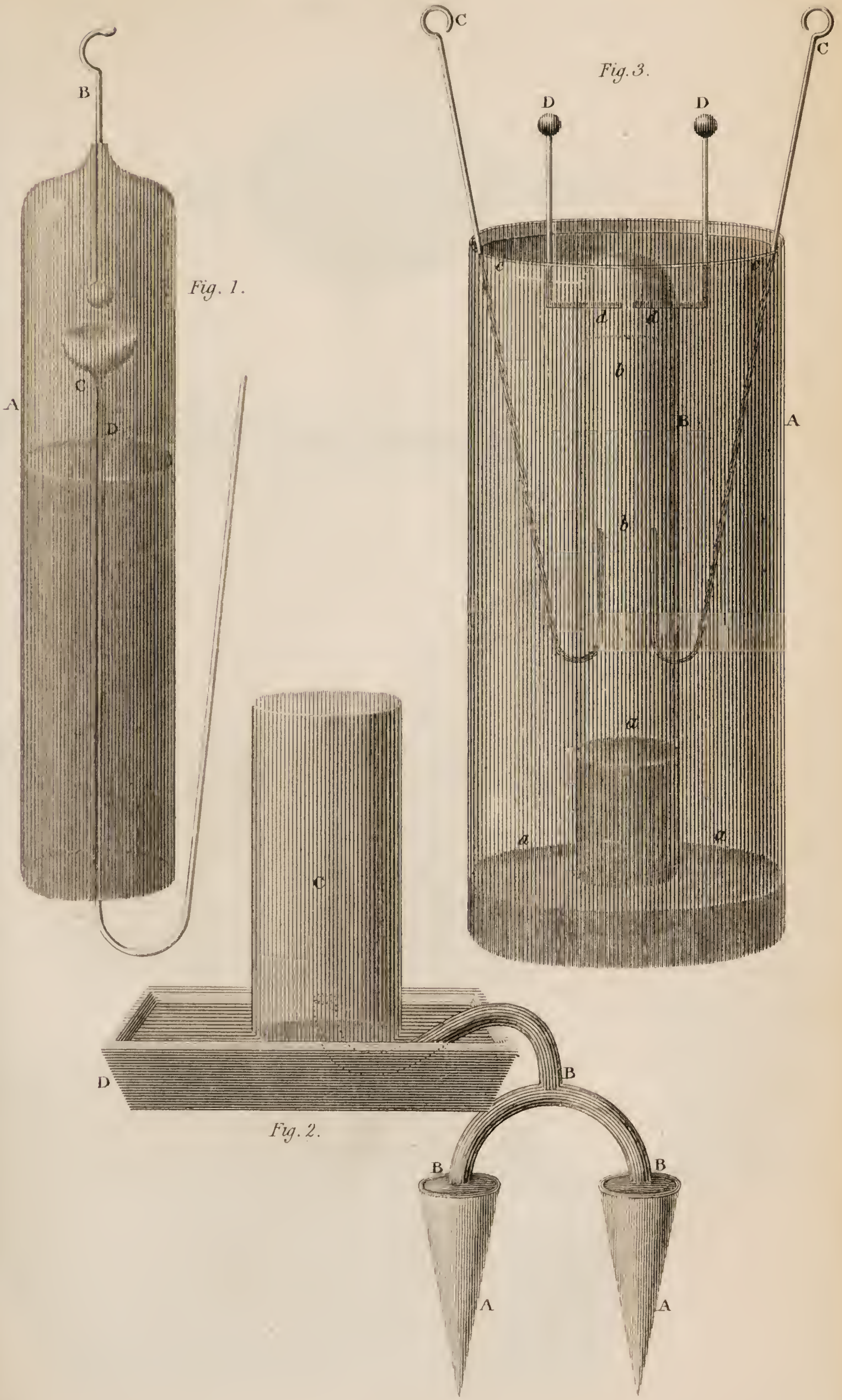


Fig. 1.

Fig. 2.

Fig. 3.

S. Porter sc.

Professor Davy's Electro-chemical Apparatus.